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ART. XXI.—*On the Variability of the Ultimate Molecule;* by Professor W. A. NORTON.

I PROPOSE, in the present article, to adduce numerous facts which seem to afford decisive evidence that the ultimate molecules of bodies, under varying conditions of pressure and other relations to surrounding molecules, are subject to change in the intensities of the forces of attraction or repulsion they are capable of exerting, at a given distance, on such contiguous molecules; while retaining the same temperature and the same chemical constitution.

The first evidence I shall present that the ultimate molecule may, under certain conditions, experience a change in the intensities of its active forces, is derived from the *facts and phenomena of the set, or permanent distortion of materials*, which remains after they have been temporarily subjected to a force of stress.

Numerous series of experiments have been made by Fairbairn, Hodgkinson, Chevandier and Wertheim, Morin, and other experimenters, the writer included, with the view of determining the amount of set experienced by various materials, after a certain fraction of the breaking load has been applied and withdrawn; and of ascertaining all the laws of the set, under varying loads, variations in the duration of stress, varying conditions in the application of repeated stresses, etc. The following are some of the principal facts and laws that have been experimentally determined.

(1.) If a bar of any material (e. g. wood, iron, or steel) be subjected to varying forces of stress—whether tensile, compres-

sive, or transverse—increasing progressively from a small fraction of the breaking load, on the removal of the load a certain set will remain, which will increase steadily with the amount of the load. The progressively increasing set will be proportional to a power of the load which varies, more or less, from one material to another, and for the same material with the ratio of the load to the breaking weight. The law of variation falls, in general, between the first and fourth power of the load; but when the stress is a large fraction of the breaking load, the set may increase according to a much more rapid law. This is strikingly true of wrought iron.

(2.) There is, strictly speaking, no definite limit of elasticity, that is, no minimum limit of the load below which no set results. This fact was decisively made out by Hodgkinson in his experiments on the behavior of wrought iron under a tensile stress, by Chevandier and Wertheim in their extensive course of experiments on numerous varieties of wood, variously strained, and in the writer's experiments on the set, or residual deflection, of bars of wood, iron and steel, after a transverse stress. In the last mentioned series of experimental determinations, a perceptible set was obtained, with each material, immediately after the stress was removed, however small its amount, until it fell below the lowest possible determination of which the apparatus was capable (viz: $\frac{1}{10000}$ of an inch); and on increasing the delicacy of the measuring apparatus it was found that the least perceptible immediate set was still limited only by the capability of detecting, with the apparatus, minute displacements.

We may say, then, that every load, however small, gives rise to an immediate set;* and that the set increases progressively with the load, however small the increments of load may be, up to the point of rupture. If for any ordinary material, there is a limiting load below which no immediate set results, such limit has not yet been determined.

(3.) The set augments with the duration of the stress, up to a certain interval of time. When moderate strains are applied it has been found that this limiting interval may vary, with the strain, from a few minutes to one hour. In experiments with white pine subjected to a transverse stress, a set from five to nine times greater was obtained by prolonging the stress.

(4.) The amount of the set does not remain invariably the same for an indefinite time after the stress is withdrawn, but is subject to material fluctuations. If very small it may pass off entirely in a few minutes. Otherwise, after decreasing for a short interval of time (from 5m. to 20m., in the author's

* By immediate set is meant that which obtains immediately after the stress is withdrawn.

experiments) it increases for a longer interval. This increase of set is eventually succeeded by another decrease; which may again be followed by other smaller fluctuations extending over several hours. As a final result the set subsides entirely, or settles into a permanent value; according to the amount and duration of the stress applied.

These facts lead unmistakably to the conclusion that the molecules of the materials experimented on, as a result of the temporary application of the force of stress, experienced an abiding change of dimensions or mechanical condition, by reason of which the intensities of their mutual actions no longer remained the same at the same distance, as before the stress was temporarily applied, and hence did not return precisely to their former configuration or relative positions; also that this molecular change increased progressively with the amount of the stress, and, to a certain extent, with the prolongation of the stress; and that the disturbed molecules, in acquiring their new mechanical condition, fluctuated more or less to the one side and the other of the final state of equilibrium. To make this more evident, let us consider somewhat in detail the results of a single series of experiments; for example, those made by Captain Rodman (U. S. Army) on a cylinder of cast iron, 35 inches long and 1·366 inches in diameter, subjected to a tensile stress. The smallest set observed was $\frac{1}{71\,4000}$ of the length, and resulted from the temporary application of a tensile stress of 6,000 pounds to the square inch. This stress, temporarily applied, had then the effect to produce an abiding increase of $\frac{1}{71\,4000}$ in the distance between two contiguous molecules in the line of stress. Loads increasing by 1,000 pounds, from 6,000 pounds to 25,000 pounds per square inch, were successively applied at considerable intervals of time, and the resulting sets, or permanent elongations, increased progressively from $\frac{1}{71\,4000}$ to $\frac{1}{28\,000}$ of the length; and accordingly the distance between contiguous molecules in the line of stress were augmented by the same fractional amounts. The direct operation of these loads, before being withdrawn, augmented the molecular distances by fractional amounts varying from $\frac{1}{37\,000}$ to $\frac{1}{38\,5}$. Now this bar of cast iron was a mass of molecules which, whenever no external stress was in actual operation, must have been in equilibrium under the operation of antagonistic forces of attraction and repulsion exerted by the molecules, and yet as the result of a temporary application of a series of increasing loads, twenty in number, it took up a series of as many different configurations, in which the distance between contiguous molecules in the line of stress augmented progressively from $\frac{1}{71\,4000}$ to $\frac{1}{28000}$; and this although the actual displacements produced by the suspended loads were only from

$\frac{1}{3700}$ to $\frac{1}{385}$. To suppose that these molecules, during all these progressive permanent changes of mutual distance, continued to exert forces of mutual action varying only with the distance, is to suppose that they were capable of taking, under the operation of such forces, constant at the same distance, an indefinite series of positions of equilibrium differing from each other by exceedingly minute fractions of the distances between the molecules. The absurdity of this supposition is still more apparent when we consider that we must also admit that the molecules were capable of being in equilibrium, in relative positions which it required at the outset certain forces of stress to bring them into, although the inherent molecular forces are not supposed to have changed. Without violating fundamental principles of Statics, we cannot escape from the conclusion that the molecules must have undergone some change, under the operation of the temporary strains developed within the bar by the external stress, by reason of which they exerted mutual actions that had no longer the same intensity, at the same distance, as before the stress was applied. To suppose that the bar was not homogeneous throughout, would here be of no avail, for whatever its inequalities of constitution or texture, at all points of its mass the equilibrium must obtain under the operation of the inherent forces of the molecules there present, whether like or unlike. Besides the fact of a progressive increase of set, by minute degrees, while the stress augments by small fractions of the breaking load, is found to hold good whether the body is in the condition of cast iron, or wrought iron, or wood (a material that has resulted from a natural growth). This statement also disposes of the conjectural explanation of the phenomena of set which has sometimes been given, that they are attributable to a relief of the particles of the body from internal strains subsisting before the stress is applied. For, if it were possible to form any distinct conception of such internal strains that could give rise to such phenomena, it is idle to suppose that all varieties of material, and all the diverse specimens of the same material, are habitually in similar states of internal strain. On the other hand, if we admit that the molecules may be susceptible of variation in their mechanical condition, under the influence of their mutual, effective attractions, or repulsions, when forced out of their relative positions of equilibrium, and that a fraction of this molecular change may abide after the external stress is withdrawn, we have a simple, general explanation of the phenomena of set, and a satisfactory basis for a definite theory of imperfect elasticity. A body which is imperfectly elastic under a certain external force of stress, would be simply one whose molecules experience a sensible permanent change of mechanical condi-

tion as the result of the operation of this force of stress. When the load is removed the body should then have a certain set.

We may add here, incidentally, that all facts and phenomena in which imperfect elasticity plays any part (e. g. the properties of ductility, malleability and plasticity), are so many additional evidences of variations occurring in the mechanical state of the molecule.

A second general evidence that the ultimate molecule is liable to variation, may be derived from the observed changes in the mechanical properties of materials produced by tension, pressure, heat, etc. Thus the enacity of iron may be greatly increased by wire-drawing, also by hammering and rolling when heated. This is generally explained by saying that the particles have been brought into new relative positions; though the effect in the latter case is attributed in part to the removal of impurities. Such an explanation is little better than a statement of the fact that a change of configuration has occurred, and the tenacity has in consequence increased. It neither states what the change of configuration is, nor why it is possible while the inherent forces of the molecules are supposed to remain the same, nor why it should be attended with an increase of tenacity.

The tenacity of bar iron may also be materially increased by the process of thermo-tension. This consists in subjecting the bar to a large tensile stress, while heated to about 400° F., and then allowing it to cool after being relieved of strain. The tenacity of good bar iron may be increased from 12 to 20 per cent by this process. At the same time the bar has become permanently elongated by about 6 per cent. The precise amount of the effect varies with the temperature to which the bar is raised, and the intensity of the tensile stress applied. Shall we assume, then, that the molecules, while retaining the same forces, have taken up an indefinite series of positions of equilibrium, varying progressively with the number of degrees of temperature and the number of pounds of stress, or admit at once that under the varying conditions of temperature and stress, permanent variations in the intensities of their active forces must have taken place, and thus new configurations have become possible, attended with augmentations of tenacity. In all such cases it must be borne in mind that the displacements of the molecules are but small fractions of the distances between them.

We will add that the well-known effects of tempering and annealing, also give intimations of permanent changes effected by variations of temperature in the mechanical condition of the individual molecules.

Let it not be understood that in what precedes, the intention

has been to convey the idea that the molecular displacements produced by wire-drawing and hammering, and the permanent changes of mechanical properties that may result from the operation for a time of a force of tension, or pressure, or a change of temperature, must be, in every instance, wholly ascribed to variations of molecular condition; but that such variations must play an important part in the mechanical processes of change, and constitute one mechanical feature of the substances thus specially affected.

Another evidence that the ultimate molecule has the property of variability, is furnished by the great change of mechanical properties that sometimes results from the presence in a body of minute quantities of other substances (e. g., the great changes in the tenacity and other properties of steel, attendant on slight variations in the percentage of carbon associated with the iron; and in the tenacity of iron or steel resulting from slight differences in the percentage of manganese, phosphorus, etc.) According to the results of the experiments of Kirkaldy on Fagersta steel (Sweden), the union of 0·5 per cent (or $\frac{1}{200}$) of carbon with the iron augmented the tenacity of the hammered bars from $\frac{1}{2}$ to $\frac{1}{5}$, and an increase in the quantity of carbon from 0·5 per cent to 1 per cent (from $\frac{1}{200}$ to $\frac{1}{100}$) augmented the tenacity from $\frac{1}{3}$ to $\frac{1}{5}$. The presence of $\frac{1}{200}$ by weight of carbon, would give only one chemical molecule of carbon for every forty-three chemical molecules of iron; and of $\frac{1}{100}$, only one molecule of carbon to every twenty-one molecules of iron. The following table, extracted from the Article on Steel, in Johnson's Cyclopedia, by Mr. A. L. Holley, gives the comparative effects on the mechanical qualities of steel of several different proportions of carbon.

Fagersta (Sweden).			Neuburg (Austria).		
Quantity of Carbon.	Av. Breaking Load.	Per centage of Stretching.	Quantity of Carbon.	Av. Breaking Load.	Per centage of Stretching.
	lbs. per sq. in.			lbs. per sq. in.	
1·00 p. c.	126, 486 to 146, 383	2 to 6 p. c.	0·83 to 1·12	126, 486 to 149, 226	5 p. c.
0·70 p. c.	100, 905 to 130, 750	4 to 6 p. c.	0·62 to 0·88	103, 747 to 126, 486	5 to 10 p. c.
0·45 p. c.	99, 404 to 103, 747	9 to 10 p. c.	0·38 to 0·62	80, 298 to 103,747	10 to 20 p. c.
0·35 p. c.	68, 217 to 69, 638	12 p. c.	0·15 to 0·38	68, 217 to 80, 298	20 to 25 p. c.
0·30 p. c.	59, 690 to 62, 533	11 to 22 p. c.	0·05 to 0·15	56, 848 to 68, 217	25 to 30 p. c.

It will be seen that with the Fagersta steel an increase in the proportion of carbon from 0·30 of 1 per cent to 1 per cent (or $\frac{1}{143}$) more than doubled the tenacity; and that with the Neu-

burg steel an increase in the proportion of carbon from 0·15 per cent to 1·12 per cent (or about $\frac{1}{100}$) augmented the tenacity in the ratio of $2\frac{1}{5}$ to 1.

It has been shown by recent experiments* that an increase in the percentage of manganese in Bessemer steel from 0·5 per cent to 1 per cent, while the percentage of carbon and phosphorus remain the same, may augment the tenacity of the steel, when untempered, by one-fifth, and when tempered in oil by one-quarter. In this case but one molecule of manganese is added to every two hundred molecules of iron.

The magnitude of these variations of tenacity is greatly disproportionate to the quantity of carbon, or manganese, associated with the iron. Nor can such mechanical effects, increasing progressively with the percentage of the ingredient, be reasonably attributed to continued variations in the number of atoms in the physical molecules, or in the configurations of unvarying molecules. It would seem that their possibility, on physical grounds, can only be conceived by admitting that the iron molecules, which combine with the carbon molecules, suffer in consequence some physical change by reason of which the intensities of their attractive actions on surrounding molecules are increased, and that these molecular changes extend, by propagation from one molecule to another, throughout the entire mass.

Additional intimations that the ultimate molecule has the property of variability, are furnished by certain facts in *Chemical Physics.*

(1.) In solution, solids assume the mechanical properties of liquids. The entire mass of the solution is in the liquid state, and, to all appearance, the molecules of the dissolved solid are as truly in the liquid condition as those of the solvent. The molecules of the solid have not suffered any change of chemical composition. The natural inference then is, that they have experienced a change of mechanical condition. The alternative supposition is that the physical molecules of the solid are more complex than the chemical molecules, and in the process of solution are broken up into others less complex, which, in their association, have the mechanical properties of a liquid.

(2.) Certain elementary substances have the property of *Allotropy*, that is of assuming, under varying circumstances, different states or forms in which they exhibit very different physical and chemical properties. Sulphur, phosphorus, oxygen, and carbon, may be cited as conspicuous examples. "Several other elements are known to be capable of existing in

* On the Effects of Phosphorus and Manganese on the Mechanical Properties of Steel; by M. Euverte (Van Nostrand's Engineering Magazine, April, 1878, p. 363, etc.).

two or more allotropic states. Indeed, instances of allotropy are so common that some chemists have been led to believe that most, if not all, of the elements may exist in distinct allotropic states." In some cases (e. g., sulphur, phosphorus and carbon) the substances occur in different states of solidification—crystalline, vitreous, or amorphous—that is, their molecules occupy different relative positions of equilibrium. Now the capability of taking up these different positions is an intimation of a probable difference in the mechanical condition of the ultimate molecules. "Diversity of crystalline structure, or its entire absence, is, however, evidently only one of the many differences of properties incidental to allotropism; in many cases it must be regarded as a consequence of the latter, by no means its cause. At all events the cases of allotropism which occur among the gases, cannot be explained by this theory" (i. e., by a mere difference of molecular arrangement.) The diverse chemical properties generally exhibited in the different allotropic states of a substance must apparently be attributable to diversities in the condition of the individual molecules; and so be regarded as evidence in support of the doctrine of the variability of molecules. In fact Berzelius suggested the term allotropism as expressive of the idea that the diversities of property observed might depend on some absolute difference of quality in the different varieties of a substance, and not upon any dissimilarity in the arrangement or number of its molecules; and we are told "that this idea has ever since steadily gained favor, although directly opposed to the doctrine of the immutability of matter, one of the principal tenets on which the chemistry of the first half of the century was based."

While in allotropism we have the significant fact that the same element can exhibit the properties of two different substances, we have in *isomerism* a kindred fact,—that the same substances combined in precisely the same proportions may form two or more compounds differing widely from each other in their chemical and physical properties. Whatever may be the supposed differences of molecular arrangement in these varying compounds, such differences must apparently find their efficient cause in varied mechanical and physical states assumed by the individual molecules of the constituent substances. That varying circumstances of combination should suffice to determine all the supposed diversities of arrangement of molecules, while the combining "chemical atoms" retain the same intensity of mutual action, seems in the highest degree improbable. In many, if not all the cases of isomerism, there must apparently be a true allotropy, or change of state of the ultimate molecule of the different elements combined.

In corroboration of the evidence of the variability of mole-

cules furnished by allotropism and isomerism, we may mention that "many well known substances exhibit differences in hardness, color, specific gravity, solubility, etc., according to the circumstances in which they are produced."

(3.) Some substances in the *nascent state* exhibit chemical properties, which they do not possess, or, if at all, only in an inferior degree, in the ordinary free state. Thus oxygen, hydrogen, and nitrogen, are much more active, chemically, in the nascent than in the free state. We must then suppose that the chemical molecules either differ in these two states, in the number of their constituent atoms, or in the physical and mechanical condition on which their chemical activities depend, without any difference in the number of constituent atoms. The former supposition involves the improbable hypothesis that the same atoms regarded as endued with inherent forces of a constant intensity, may take up two or more different relative positions of equilibrium.

The citation of facts, mechanical, physical, and chemical, which furnish evidence that the ultimate molecule is liable to variation, might be extended almost indefinitely. In fact, in every change of state which a body may experience, and in almost every change of physical relation to other substances, the active forces exerted by the molecules on one another are different in intensity—though the temperature remain the same; and, in general, after the body has been subjected to any mechanical process, the molecules manifest different intensities of mutual action. Many physical processes are attended with similar results. Some of these variations of molecular condition have been conjecturally explained by the vague hypothesis that the particles are differently arranged—either that the existing molecules, regarded as groups of kindred atoms, take on different configurations, or become broken up and replaced by others. But others are of such a character that this explanation is wholly inadmissible (see p. 185); and, in general, no definite theory is offered of a mechanical process by which such supposed changes in the constitution of the molecules may be brought about.

If, in view of the array of evidence that has now been presented, it be admitted that the ultimate molecule has the property of variability, in the sense that has been defined, under varying mechanical relations to other molecules, and the molecule be regarded merely as a group of kindred atoms, then we must conclude that *these atoms are also variable*, after the same manner as the molecule itself. For it is obvious that a group of kindred atoms cannot exercise an external action varying in intensity at a given distance, and increasing progressively with the amount of change experienced in its mechanical relations

to surrounding molecules, if the inherent atomic forces suffer no change. But to suppose that the inherent atomic forces vary in intensity at a given distance, is to discard the ordinary conception of the atom, which is that it is not only invariable in its mass and volume, but also in the intensity of action, whether attractive or repulsive, it is capable of exerting on another atom at a given distance from it. We are accordingly constrained to regard the elementary parts of molecules, which have received the designation of atoms ("chemical atoms") as in reality liable to variation in their capabilities of mechanical action. But we are not therefore under the necessity of rejecting the fundamental idea of invariable atoms, for we may instead regard the "chemical atom" as consisting of a true atom of ordinary matter invested with an ethereal atmosphere, and thus as being an organized mechanical system, that may be capable of variation in its dimensions and force of external action at the unit of distance, with varying external relations—though the inherent forces of all the atoms in the system remain unchanged. This is the general conception of the "chemical atom" which I have adopted in my papers on Molecular Physics.* Upon this view the chemical atom, and the molecule (chemical or physical) have the same general constitution, and differ only in the precise mechanical state of the ethereal atmosphere that invests the atom proper. To this last element of bodies, incapable of change in its mass as measured by its weight, I have given the designation of the *ultimate molecule.* The mutual actions of ultimate molecules constitute the *molecular forces;* and so include, taken in the most comprehensive sense, the mutual actions of ultimate molecules in the different state answering to the "chemical atom," "chemical molecule," and "physical molecule." Upon this hypothesis, with regard to the constitution of the chemical atom and molecule, it is the investiture of the atom proper with an ethereal atmosphere that has armed it with the power of operating on adjacent atoms; and, at the same time, imparted to it its properties with respect to the physical agents of light, heat and electricity. The definite conception I have formed of the constitution of this ethereal atmosphere, is not that it is simply a mass of luminiferous ether condensed around the atom. Such an atmosphere, perfectly elastic, would be incapable of permanent change as a consequence of temporary variations of external pressure. Besides, while researches in Physical Optics have led to the conclusion that the atoms of bodies are probably surrounded by such atmospheres, electric phenomena give intimation of the presence in bodies, and in intimate association with their molecules, of a subtile fluid termed the "electric

* This Journal, July, 1864, and May, 1872.

fluid," or ether. It has been shown by physicists that certain of these phenomena do not absolutely require the admission of an ether distinct from the luminiferous, but the same cannot be said of electric phenomena in general. Moreover, all the efforts hitherto made to explain electric phenomena by hypothetical motions of the atoms of bodies, have proved futile. No definite physical theory of electricity has been framed that does not involve the conception of a subtile electric fluid, the atoms of which repel each other, and are attracted by those of ordinary bodies; and we have as yet no sufficient reason for abandoning the original hypothesis that this fluid is distinct from the luminiferous ether. If, then, there be an electric ether distinct from the luminiferous and presumably less subtile (i. e. made up of larger atoms) the ethereal atmosphere condensed around an atom by its attractive action, should consist of an atmosphere of luminiferous ether and an *envelope* of electric ether immersed within this for a certain depth. Such is the definite conception I have adopted of the constitution of the molecular atmosphere. It may be characterized as an ethereo-electric atmosphere. From this combined with the fundamental hypothesis that recurring impulses are incessantly exerted on one another, by all the atoms, ethereal and non-ethereal, that make up the ultimate molecule, I have deduced the operation of certain molecular forces. The two ethers condensed around the atoms, and pervading the interstices between them, are constituted as media capable of transmitting wave actions, by the permanent statical repulsion exerted between their atoms. The recurring impulses just mentioned originate such wave actions which are propagated from one molecule to another. The attractive impulses exerted by the central atom of a molecule on its electric envelope, originate in it waves that take effect attractively on contiguous molecules. On the other hand, the repulsive impulses exerted by the atoms of the envelope on each other, originate waves that take effect repulsively on these molecules. In considering the action of one molecule on another contiguous to it, the virtual center of the first system of waves will be somewhat without the inner surface of the envelope, and that of the other system somewhat within its outer surface—both centers being on the line of the centers of the two molecules. It is farther to be observed, that the subsidences of the envelope, induced by the attractive impulses from the central nucleus, should originate waves in the dense luminiferous ether, posited below the envelope, which would be propagated indefinitely outward and take effect on the contiguous molecule as repulsive pulses. But this system of wave-actions may be combined with the attractive system into resultant attractive actions. The different

sets of waves, emanating from different virtual centers, will be propagated according to the law of inverse squares. The *effective* action of one molecule on a contiguous one will be the difference between the resultant attractive action just mentioned, and the opposing action of the system of repulsive waves first mentioned. The following is the general expression for this *force of effective molecular action.**

$$f = \frac{n(3r^2+2rx)}{(r+x)^3(2r+x)^3} - \frac{m}{x^3}, \qquad (1)$$

in which x denotes the distance between the molecular envelopes, r the distance between the center of the system of repulsive waves first mentioned and that of the system of attractive waves, n the coëfficient of attraction, and m that of repulsion. If we put $x=ur$, $\frac{n}{m}=k$, and $\frac{m}{r^3}=p$, this expression becomes

$$f = \left(\frac{k(3+2u)}{(1+u)^3(2+u)^3} - \frac{1}{u^3}\right)p. \qquad (2)$$

The first term represents the attractive action, the other the repulsive. I propose, in a subsequent paper, to apply to this physical formula numerous quantitative and qualitative tests.†

From the point of view taken in the present discussion the important question here arises, whether the ultimate molecule as it has been defined, endued with the forces of external action just specified, is susceptible of permanent variation, and if so, how? It is, in fact, not difficult to see that it may be subject to variation in its dimensions, and in the coëfficients (n and m) of its attractive and repulsive wave impulses, under variations of pressure. Suppose that the external pressure is increased, and the ultimate molecules are thus urged nearer to each other, the molecular envelopes will in consequence be compressed, and the dimensions of the molecules, as well as their coëfficients of attraction and repulsion be altered. When the compressing force is withdrawn, if the dense lumiiniferous ether posited between the envelopes and the central atoms of the ultimate molecules has been merely compressed without any diminution of its quantity, the envelopes should return to their original position, and the coëfficients of attractive and repulsive wave actions to their original value, and the molecules would return to their original relative positions; but in fact a portion of this dense ether will be urged outward between the atoms of the envelopes, and so when the recoil comes on, the

* See this Journal, July, 1864, p. 68, and May, 1872, p. 338.

† A paper embodying the principal results obtained in the application of the tests here referred to, was read before the National Academy of Sciences at the meeting in April last.

envelopes may fail to reach their original distance from the central atoms. The dimensions of the molecules, as a whole, may thus be permanently diminished, and, in consequence, the distance between the centers of contiguous molecules at which the opposing molecular actions counterbalance each other be permanently less. The coëfficients n and m (eq. 1) should also be more or less altered, and thus the neutral distance x between the contiguous envelopes at which the effective force f becomes zero, should vary. From the combined operation of these two causes, the relative positions of equilibrium of the molecules should be more or less altered. If a force of tension be applied to the body, a permanent change of an opposite character may ensue.

It still remains to be seen how far the molecular theory that has been set forth, can be reconciled with chemical facts. This question opens too wide a field for present discussion even in the most cursory manner, but a word or two ought to be said to obviate the objection that may at once occur to the reader as fatal to the theory, viz: that the absolute invariability of atoms is established by chemical facts. Strictly speaking, these facts only show the weight of the atoms to be invariable, and that they exhibit the same chemical properties whenever the relations to other atoms are the same, and also in certain cases (e. g. solution) over a certain range of variation in such relations; provided, also, the operation of the physical agents on them is the same. But this does not preclude the supposition that large variations of their mechanical state may occur while the atoms are under different mechanical and physical relations. It may be added that the received molecular formulas of substances would still remain the same, but would have a different physical interpretation. The hypothesis of the breaking up of complex molecules and the formation of new ones, would be replaced by the simple conception of the contraction or expansion of molecular envelopes, with attendant variations of the volume of the mass, and in the physical and chemical properties of the ultimate molecules, which now play the part of "chemical atoms." In gaseous compounds the number of atoms specified in the molecular formula of the compound, would represent the proportionate degree of condensation of the gaseous mixture. Thus, when two volumes of hydrogen are mixed with one volume of oxygen, and by the electric spark are made to combine, the mixed gases would be condensed, by reason of a condensation of all the molecular envelopes, and thus a diminution in the size of the effective molecules, into a space two-thirds that occupied by the mixture before the combination. The elementary volume of the mass would contain two ultimate molecules ("chemical atoms")

of hydrogen, and one of oxygen, just as the "molecular volume," in the received chemical theory, contains these atoms.

If we contemplate this general conception of chemical phenomena, deducible from our general molecular theory, simply in the light of a representative scheme, it must be admitted to be far simpler than the received theory; which is, for the most part, only a scheme of hypothetical transformations supposed to be in some inexplicable way effected by a force of "affinity" or "chemism." For it regards the molecule and "chemical atom" as having a similar physical constitution, and attributes all changes of chemical properties occurring in such molecules to contractions or expansions of their ethereal envelopes, and attendant variations in their active forces, under the operation of the molecular forces and physical agents. If it should hereafter be made apparent that the mechanical relations of *dissimilar* ultimate molecules may in fact be such that changes in the dimensions and effective forces of the molecules may supervene, and that a special force of attraction may come into operation when two such molecules come into juxtaposition, it will be seen that a rational basis is offered for a physical theory of chemical phenomena.

Yale College, Oct. 25, 1878.

Art. XXII.—*Möbius on Eozoon Canadense*;* by J. W. Dawson, LL.D., F.R.S.

Eozoon Canadense has, since the first announcement of its discovery by Logan in 1859, attracted much attention, and has been very thoroughly investigated and discussed, and at present its organic character is generally admitted. Still its claims are ever and anon disputed, and as fast as one opponent is disposed of, another appears. This is in great part due to the fact that so few scientific men are in a position fully to appreciate the evidence respecting it. Geologists and mineralogists look upon it with suspicion, partly on account of the great age and crystalline structure of the rocks in which it occurs, partly because it is associated with the protean and disputed mineral Serpentine, which some regard as eruptive, some as metamorphic, some as pseudomorphic, while few have had enough experience to enable them to understand the difference between those serpentines which occur in limestones, and in such relations as to prove their contemporaneous deposition, and those which may have resulted from the hydration of olivine or

* Der Bau des Eozoon Canadense, von Karl Möbius, Professor der Zoologie in Kiel. Palæontographica, Band xxv.

similar changes. Only a few also have learned that *Eozoon* is only sometimes associated with serpentine, but that it occurs also mineralized with loganite, pyroxene, dolomite, or even earthy limestone, though the serpentinous specimens have attracted the most attention, owing to their beauty and abundance in certain localities. The biologists on the other hand, even those who are somewhat familiar wtth foraminiferal organisms, are little acquainted with the appearance of these when mineralized with silicates, traversed with minute mineral veins, faulted, crushed and partly defaced, as is the case with most specimens of *Eozoon*. Nor are they willing to admit the possibility that these ancient organisms may have presented a much more generalized and less definite structure than their modern successors. Worse, perhaps, than all these, is the circumstance that dealers and injudicious amateurs have intervened, and have circulated specimens of *Eozoon*, in which the structure is too imperfectly preserved to admit of its recognition, or even mere fragments of serpentinous limestone, without any structure whatever. I have seen in the collections of dealers and even in public museums, specimens labelled "*Eozoon Canadense*" which have as little claim to that designation as a chip of limestone has to be called a coral or a crinoid.

The memoir of Professor Möbius affords illustrations of some of these difficulties in the study of *Eozoon*. Professor Möbius is a zoologist, a good microscopist, fairly acquainted with modern foraminifera, and a conscientious observer; but he has had no means of knowing the geological relations and mode of occurrence of *Eozoon*, and he has had access merely to a limited number of specimens mineralized with serpentine. These he has elaborately studied, and has made careful drawings of portions of their structures, and has described these with some degree of accuracy; and his memoir has been profusely illustrated with figures on a large scale. This, and the fact of the memoir appearing where it does, convey the impression of an exhaustive study of the subject, and since the conclusion is adverse to the organic character of *Eozoon*, this paper may be expected, in the opinion of many not fully acquainted with the evidence, to be regarded as a final decision against its animal nature. Yet, however commendable the researches of Möbius may be, when viewed as the studies of a naturalist desirous of satisfying himself on the evidence of the material he may have at command, they furnish only another illustration of partial and imperfect investigation, quite unreliable as a verdict on the questions in hand. The following considerations will serve to indicate the weak points of the memoir.

1. A number of errors and omissions arise from want of study of the fossil *in situ*, and from want of acquaintance with

its various states of preservation. Trivial errors of this kind are his referring to my photograph in Plate III, of the "Dawn of Life," as if it were natural size, and his stating that the larger specimens have fifty laminæ, whereas they often have more than an hundred. More important is his failing to appreciate aright the occurrence of *Eozoon* in certain layers of regularly bedded limestones, the rounded or club-shaped forms of the more perfect specimens, the manner in which the layers become confluent at the edges of the forms, as described by Sir W. E. Logan and myself, or the amount of crushing and fracture which most of the specimens exhibit. Thus he fails to convey any adequate idea of the Stromatoporoid forms and mode of occurrence of the organism, or indeed of its general character and probable mode of growth. Farther he treats it from the first as a mere laminated aggregate of calcite and serpentine, without reference to its occurrence in any other state, and also without reference to the fragmental limestones in part made up of its remains. He objects strongly to the want of definiteness of form and distribution in the chambers and connecting passages, without making allowance for defects of preservation, or mentioning the similar want of defined form in some *Stromatoporæ*. He admits, however, that the modern *Carpenteria* and its allies are in some respects equally indefinite. He farther objects to the impossibility of detecting regular primary chambers like those in modern foraminifera, but seems not to be aware that, as I have recently shown, some *Stromatoporæ* originate in a vesicular, irregular mass of cells, and that in *Loftusia*, both the Eocene *L. Persica*, and the Carboniferous *L. Columbiana*, the primary chamber is represented by a merely cancellated nucleus.*

2. With reference to the finely tubulated proper wall of Eozoon, he has fallen into an error scarcely excusable in an observer of his experience, except on the plea of insufficient access to specimens. He confounds the proper wall with the chrysotile veins traversing many of the specimens, and obviously more recent than the bodies whose fissures they fill. That he does so is apparent from his stating that the proper-wall structure sometimes crosses the bands of serpentine and calcite, and also that it presents a series of parallel four-sided prisms, whereas, when at all perfectly preserved, it shows a series of cylindrical threads penetrating a calcite wall. That some of his specimens have contained the proper wall fairly preserved is obvious from his own figures, in which it is possible to recognize both this structure and chrysotile veins, though confounded by him under the same designation. He objects, somewhat naïvely, that many of the chambers fail to exhibit

* See Journal of London Geol. Soc., January, 1878.

this nummuline wall, and that it sometimes presents a ragged appearance or is altogether opaque. In point of fact it can appear distinctly, either in decalcified specimens or in slices, only when the minute tubes are filled with some substance optically distinguishable from calcite, or not acted on by dilute acid. When the proper wall is merely calcareous (and I have specimens showing that it is often in this state, and without any serpentine in its pores), its structure is ordinarily invisible, and it is the same when the calcareous skeleton has from any cause lost its transparency or has been replaced by some other mineral substance. Even in thickish slices, the tubes, though filled with serpentine, may be so piled on one another as to be indistinct. All this may be seen in Tertiary *Nummulites*. When wholly calcareous their tubulation is often quite invisible, and when imperfectly injected with glauconite or other silicates, they often present a very irregular appearance. If Professor Möbius will study the Nummulites injected with glauconite from Kempten,* Bavaria, in addition to the casts of *Polystomella* from the Ægean to which he refers, he will be better able to appreciate these points. It may be worth repeating here that, in examining the original specimens of Eozoon, I did not recognize the proper wall. I did not doubt that it must have existed in some form, since I could easily detect the canals in the supplemental skeleton; but I did not wonder at its non-appearance, knowing the chances against its preservation in a recognizable form. Its discovery was due to the subsequent investigations of Dr. Carpenter.†

3. To the canal system, Professor Möbius does more justice, and admits its great resemblance to the forms of this structure in modern *Foraminifera*. This indeed appears from his own figures, as will be seen from the fac-simile tracings reproduced here, figs. 1, 2, 3 and 4, which well show how wonderfully this structure has been preserved, and how nearly it resembles the similar parts of modern *Foraminifera*. He thinks, however, that these round and regularly branching forms are rather exceptional, which is a mistake; though it is true that the sections of the larger canals are often somewhat flattened, and that they become flat where they branch. They are also sometimes altered by the vicinity of veinlets or fractures, or by minute mineral segregations in the surrounding calcite, accidents to which all similar structures in fossils are liable. Another

*I am indebted to Dr. Otto Hahn for specimens of these most interesting fossils.

†It may deserve mention here that the Carboniferous *Fusulina* very rarely shows its tubulated wall, and that Dr. Carpenter had maintained its Nummuline affinities before he obtained specimens showing this particular structure. Structures so delicate as these are indeed only preserved exceptionally in fossil specimens.

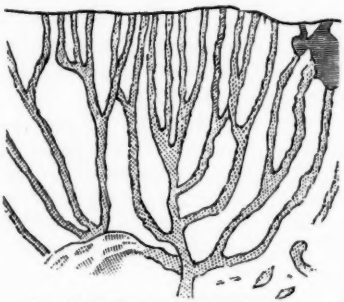
objection, not original with him, is derived from their unequal dimensions. It is true that they are very unequal in size, but there is some definiteness about this. They are larger in the thicker and earlier formed layers, smaller or even wanting in the thinner and more superficial. In some slices the thicker trunks only are preserved, the slender branches having been filled with dolomite or calcite. It is difficult, also, to obtain, in any slice or any surface, the whole of a group of canals.* Farther, as I have shown, the thick canals sometimes give off groups of very minute tubes from their sides, so that the coarser and finer canals appear intermixed. These appearances are by no means at variance with what we know in other organic structures. Another objection is taken to the direction of the canals, as not being transverse to the laminæ but oblique. This, however, may be dismissed, since Möbius has of course to admit that it is not unusual in modern *Foraminifera*. It may be added that some of the appearances which puzzled Möbius, and which are represented in his figures, evidently arise from fractures displacing parts of groups of canals, and from the apparently sudden truncation of these at points where the serpentine filling gives place to calcite. It would also have been well if he had studied the canal systems of those *Stromatoporæ* which have a secondary or supplemental skeleton, as *Cœnostroma* and *Caunopora*. In illustration of this I give in fig. 5 a group of these canals from a recent paper of my own.†

4. A fatal defect in the mode of treatment pursued by Möbius is that he regards each of the structures separately, and does not sufficiently consider their cumulative force when taken together. In this aspect, the case of *Eozoon* may be presented thus: (1.) It occurs in certain layers of widely distributed limestones, evidently of aqueous origin, and on other grounds presumably organic. (2.) Its general form, lamination and chambers, resemble those of the Silurian *Stromatopora* and its allies, and of such modern sessile foraminifera as *Carpenteria* and *Polytrema*. (3.) It shows under the microscope a tubulated proper wall similar to that of the Nummulites, though of even finer texture. (4.) It shows also in the thicker layers a secondary or supplemental skeleton with canals. (5.) These forms appear more or less perfectly in specimens mineralized with very different substances. (6.) The structures of *Eozoon* are of such generalized character as might be expected in a very early Protozoan. (7.) It has been found in various parts of the world under very similar forms, and in beds approximately of the same geological horizon. (8.) It may be

* I have succeeded best in this by etching the surface of broken specimens.

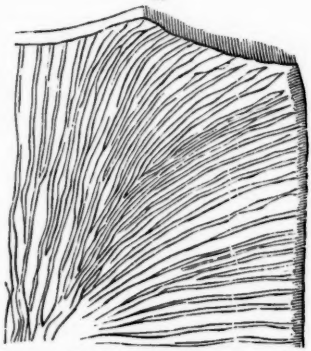
† Journal of London Geological Society, January, 1878.

1.



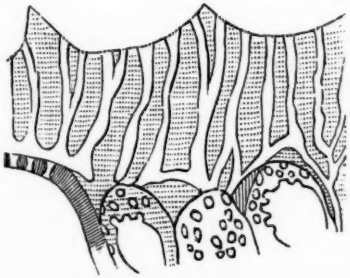
Canals of *Eozoon* (after Möbius).

2.



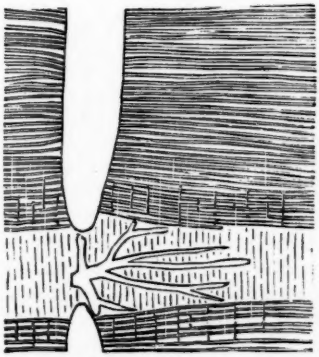
Finer canals of *Eozoon* (after Möbius).

3.



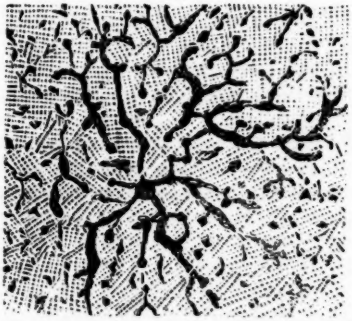
Canals of modern *Calcarina* (after Möbius).

4.



Canals and Tubule of Tertiary *Nummulina* (after Möbius).

5.



Canals of *Cœnostema*—Upper Silurian—(original).

added, though perhaps not as an argument, that the discovery of *Eozoon* affords a rational mode of explaining the immense development of limestones in the Laurentian age; and on the other hand that the various attempts which have been made to account for the structures of *Eozoon* on other hypotheses than that of organic origin have not been satisfactory to chemists or mineralogists, as Dr. Hunt has very well shown.

Professor Möbius, in summing up the evidence, hints that Dr. Carpenter and myself have leaned to a subjective treatment of *Eozoon*, representing its structure in a somewhat idealized manner. In answer to this it is necessary only to say that we have given photographs, nature-prints and camera tracings of specimens actually in our possession. We have not thought it desirable to figure the most imperfect or badly preserved specimens, though we have taken pains to explain the nature and causes of such defects. Of course, when attempts at restoration have been made, these must be taken as to some extent conjectural; but so far as these have been attempted they have consisted merely in the effort to eliminate the accidental conditions of fossilized bodies, and to present the organism in its original perfection. Such restorations are not to be taken as evidence, but only as illustrations to enable the facts to be more easily understood. It is to be observed, however, that in the study of such fossils as *Eozoon*, the observer must expect that only a small proportion of his specimens will show the structures with any approach to perfection, and that comparison of many specimens prepared in different ways may be necessary in order to understand any particular feature. A single figure or a short description may thus represent the results of days spent in the field in collecting, of careful examination and selection of the specimens, of the cutting of many slices in different directions, and of much study of these with different powers and modes of illumination. My own collection contains hundreds of preparations of *Eozoon*, each of which represents perhaps hours of labor and study, and each of which throws some light more or less important on some feature of structure. The results of labor of this kind are unfortunately very liable to be regarded as subjective rather than objective by those who arrive at conclusions in easier ways.

Taken with the above cautions and explanations, the memoir of Professor Möbius may be regarded as an interesting and useful illustration of the structures of Eozoon, though from a point of view somewhat too limited to be wholly satisfactory.

Art. XXIII.—*The Magnetic Storm of May* 14, 1878, *observed in North America.*

[Communicated by Carlile P. Patterson, Superintendent United States Coast and Geodetic Survey.]

The extensive magnetic disturbance of May 14, 1878, of which accounts have been given in Nature,* and which was observed in China, Australia and England, also made its record in North America at our magnetic observatory, established at Madison, Wisconsin, in the winter of 1876–1877. This observatory is in latitude 43° 04′ 29″·5, and in longitude 5^h 57^m 36^s·5 west of Greenwich; in it are mounted a set of Brooke's Magnetographs, and daily photographic traces of the changes in magnetic declination and in the horizontal and vertical forces have been produced since March, 1877, and are intended to be kept up for some years. The declination traces for several days preceding the 14th were normal, but about midnight May 13–14, a series of disturbances commenced consisting in part of some large oscillations to the eastward and westward, and in part of a great number of small and rapid oscillations. The characteristic features of the trace may be given as follows:

	Madison mean time. d. h. m.		Greenwich mean time. d. h. m.	
The disturbance in *declination* commenced about	14 0 00	A. M.	14 5 58	A. M.
A principal westerly† extreme reached	1 05	"	7 03	"
A principal easterly extreme reached	2 36	"	8 34	"

Range of motion 16′·5; after this a series of smaller oscillations continue to past noon.

	d. h. m.		d. h. m.	
A maximum westerly position is reached about	0 16	P. M.	6 14	P. M.
And an easterly extreme at	0 53	"	6 51	"
Extreme westerly deflection at	3 26	"	9 24	"
A sharp motion to the eastward commences at	5 40	"	11 38	"
A principal easterly extreme reached at	6 24	"	15 0 22	A. M.

Range of motion of principal disturbance 31′·7.

There is also a westerly extreme about	7 10	"	1 08	"
And an easterly extreme about	9 16	"	3 14	"

After 10 P. M. the irregularities gradually subside.

Last extreme easterly position	15 1 10	A. M.	7 08	"

It will be noticed that at the Greenwich observatory the storm commenced on May 14, at 6^h 05^m A. M., at Stonyhurst observatory at 6^h 04^m, at Zi-ka-wei, near Changhai, also at 6^h 04^m (Gr. time), and at Melbourne supposed at 6^h 20^m; the storm may therefore be taken as simultaneous at these places. At

* Vol. xviii, Nos. 467, 468, 469. † Referring to north end of the magnet.

Greenwich the north end of the needle moved eastward between 6 and 9 A. M., but at Madison the general motion was westerly; again the sharp deflection commencing at $5^h\ 40^m$ Mad. time ($11^h\ 38^m$ P. M. Gr. time) was to the *eastward* at Madison, and to the westward after $11^h\ 45^m$ at Greenwich, thus deflecting the magnets in opposite directions.

The northern component of the *horizontal* force was sharply affected at Madison, the force diminishing at $14^d\ 0^h\ 05^m$ A. M., Madison time, $14^d\ 6^h\ 03^m$ A. M., Greenwich time. The disturbance continued, but between $2\frac{1}{2}$ and $7\frac{1}{2}$ A. M., the trace is too indistinct to be read; the small oscillations continue to about $11^h\ 45^m$, when they become superseded by a series of larger waves culminating in a maximum extreme at $2^h\ 30^m$ P. M., Madison time, $8^h\ 28^m$ P. M., Greenwich time, and a minimum at $3^h\ 58^m$ P. M., Madison time, $9^h\ 56^m$ P. M., Greenwich time. The large disturbances continue till about $10^h\ 20^m$ P. M., having reached a maximum extreme about $4^h\ 50^m$ P. M., Madison time, $10^h\ 48^m$ P. M., Greenwich time, and a minimum extreme at $9^h\ 03^m$ P. M., Madison time; $15^d\ 3^h\ 01^m$ A. M., Greenwich time. Range between maximum at $2^h\ 30^m$, and minimum at $9^h\ 03^m$, $\frac{1}{175}$ of the horizontal force, nearly. In the Greenwich account it is stated "the first start in the trace of the declination magnet at $18^h\ 05^m$ (Astr. Rec.) is most distinct." Now within two minutes of this time occurs the first and sharpest deflection in our horizontal force trace at Madison, thus marking distinctly the commencement and simultaneousness of the storm.

The disturbance in the *vertical* force commenced about May 14, $1^h\ 10^m$ A. M. ($7^h\ 08^m$ Gr. time), and terminated about $3^h\ 30^m$ A.M.; between this time and $5^h\ 45^m$ P. M., the trace was smooth, but between $5^h\ 45^m$ and $6^h\ 45^m$ a sharp deflection took place in the opposite direction, the maximum force occurring at $6^h\ 11^m$ P.M. (May 15, $0^h\ 09^m$ A. M. Gr. time), the deflection or increase amounted to $\frac{1}{125}$ of the vertical force. The vertical force trace did not exhibit any of the tremulous motion noticed in the other two curves.

I may state that a description of the Madison magnetic observatory, together with the discussion of the first year's observations and results, is nearly ready in manuscript for publication.

CHAS. A. SCHOTT,
Assistant C. and G. S. in charge Observatory.

Coast Survey Office, Washington, D. C., Jan. 6, 1879.

ART. XXIV.—*On the Flocculation of Particles, and its Physical and Technical Bearings;* by EUGENE W. HILGARD, of the University of California.

I HAVE heretofore (this Journal, Oct. and Nov., 1873) discussed, casually rather than directly, the tendency of small particles to form, under the influence of a moderate agitation, granular aggregates or compound particles of larger size, opposing to mechanical disintegration more or less resistance, according to the circumstances of temperature and moisture. Some discussions since made, and more especially that of Professor S. W. Johnson, on the Mechanical Effects of Tillage,* appear to me to render desirable a more detailed consideration of the various bearings of this phenomenon, which, despite its simplicity, and obviousness in every-day life, seems not to have received all the attention it deserves. This is strikingly exemplified in the modes of experimentation and the more or less contradictory results obtained by different observers—Von Klenze, Haberlandt, Ad. Mayer, Nessler, and others who have studied the capillary relations of soils to water. In comparing the effects of "packing" and "loosening" of powders and soils upon their relations to water, it makes a material difference whether the material used was dry, damp or wet, and whether or not the soil had been previously under tillage for some length of time. Again, Johnson seems to incline to attribute to the flocculation of the *clay* in soils, by various agencies, effects which, I think, clearly belong to all small particles as such.

Perhaps the best fundamental experiment (which can be neatly projected on a screen by the beam of a magic lantern) is the one described in my previous paper (loc. cit., p. 290). A sediment consisting of granules of the uniform hydraulic value of 1^{mm} per second, is introduced into an ordinary conic-cylindrical elutriating tube, placed vertically, in which the current of water entering at the small orifice below, alone performs the stirring-up needed to keep the sediment from settling down. If now a current of water be turned on, corresponding to a velocity *below* that of 1^{mm} at the mouth of the tube, of course none of the sediment can pass out of the upper end, but it will be kept circulating in the conical portion, by a current going upward in the axis of the tube, and downward on the sloping sides of the cone. If this be kept up for ten or fifteen minutes, and then the velocity gradually increased, it will be found that scarcely any of the sediment will pass off, either at the

* Rep. Conn. Exp. Station for 1877.

velocity corresponding to its true hydraulic value, or even at one four or five times higher; the cause being that its single grains have coalesced or "flocculated" into compound grains of from five to thirty or more of the original ones, thus forming in effect a very coarse sediment of roundish masses. When these are allowed to settle, it will be seen that this altered sediment occupies a very much larger space than did the original single-grain sediment, in consequence of the large vacant spaces left between the aggregate-grains; the relation between the two being somewhat like that between gunpowder dust and coarse blasting powder.

If now the coarse sediment be violently stirred by shaking up, or if it be boiled even lightly, the heavy masses disappear, the water becomes uniformly turbid, and after the proper time of quiescence, a thin horizontal layer of single-grain sediment is formed at the bottom of the vessel.

There is nothing very remarkable or new in this experiment; every chemist knows the phenomena it exhibits from every-day experience, and acts upon it in the gentle stirring given to liquids in which precipitates just forming are desired to flocculate. Every geologist has seen the flocculent aggregate-granules rolled along the bottom by a depositing current, and has noticed the rapid clearing of the gentle stream after it has been rendered turbid by stirring; while quiescent pools *in the same material* have remained turbid for some length of time. The experiment with the elutriator-tube, however, is well adapted to the demonstration of the following laws governing the formation of aggregates of immersed particles:

1. The tendency to flocculation is roughly in inverse ratio to the size of the particles. I have already stated (loc. cit., p. 291) that with quartz grains it practically ceases when their diameter exceeds about $0{\cdot}2^{mm}$, or 8^{mm} hydraulic value. It goes without saying that the number of particles that may cohere into one mass, follows substantially the same law. Sediment of $0{\cdot}25^{mm}$ hydraulic value, will sometimes form large masses, like snow flakes, on the sides of the elutriator-tube.

2. The degree of agitation which will resolve the aggregates into single grains, is (roughly) inversely as the size of the particles; or more properly, perhaps, inversely as their hydraulic value.

3. The tendency to flocculation varies inversely as the temperature. In water near the boiling point it is very slight; so strikingly less, that at one time I seriously contemplated the use of such water in the mechanical analysis of soils, in lieu of the mechanical stirring. The difficulties of construction and manipulation of course render this method undesirable, even if it were altogether efficient.

4. The presence of alcohol, ether, and of *caustic* or *carbonated alkalies*, materially diminishes the tendency to flocculation; while the presence of acids and neutral salts seems to increase it.

5. As between sediments of equal hydraulic value but different density, the tendency to flocculation seems to be greater with the less dense particles.

My object being, at the time, not so much the determination of a question of abstract physics, but the finding of an accurate and practicable method of mechanical soil-analysis, I have not pursued farther the interesting points suggested by 4 and 5. The observations made, however, rendered quite obvious a close relation between the capillary properties of liquids and the flocculative tendency of sediments suspended in them. Just what this relation is, must be determined by farther experiment; nor will I undertake to discuss at this time what is the precise nature of the attraction that causes the formation of these flocculated aggregates, and when formed holds them together. Regarding the single particles as irregular spheroids, each of which can, at best, come in contact at three points with any other particle, it cannot be mere surface adhesion independent of the liquid; and the particles being submerged, there is no meniscus to create an adhesive tension. Since experiment shows that the flocculative tendency is measurably governed by the cohesion-coëfficient of the liquid, it seems necessary to assume that capillary films of the latter interposed between the surfaces of solids, *do* create a considerable adhesive tension even in the absence of a meniscus; an assumption that would seem to be rendered necessary also by the enormous increase of adhesion caused by the interposition of a solid film of liquid between plane plates, even when submerged, as compared with that of, e. g., two watch glasses adhering only by their wetted edges, with two concentric circles of menisci. *A priori*, it is to be supposed that the surface tension known to exist between two liquid surfaces, must exist, and exert a corresponding influence between the surfaces of solids and liquids, apart from any meniscal action. I am unable to find that any exhaustive investigation of this particular phase of the subject of molecular attraction has as yet been made.

It is of course to be expected, that the adhesion of the particles constituting one of these floccules will be very materially increased whenever the formation of menisci between them becomes possible by the removal of the general liquid mass. Suppose one of the floccules to be "stranded," it will in the first place remain immersed in a sensibly spherical drop of liquid. As this liquid evaporates, the spherical surface will become pitted with menisci forming between the single projecting

particles; and as these menisci diminish their radius by still farther evaporation, the force with which they hold the particles together will increase, until it reaches a maximum, the position of which (expressed in the liquid-percentage of the mass) must be sensibly a function of the size of the constituent particles. As the evaporation progresses beyond this point of maximum, the adhesion of the constituent particles must diminish by reason of the disappearance of the smaller menisci; and when finally the point is reached when liquid water ceases to exist between the surfaces, the slightest touch, or sometimes even the weight of the particles themselves, will cause a complete dissolution of the floccule, which then flattens down into a pile of single granules.

All this can readily be observed by any one in the progress of a mechanical soil analysis; provided only that the complete separation of the clay from the siliceous sediments has been previously accomplished in the manner set forth in my former paper. The presence of flocculated or coagulated clay modifies the phenomena, in so far as it tends to hold the floccules together under circumstances that in its absence would have caused a complete collapse.

I now proceed to discuss some of the obvious bearings of the phenomenon of flocculation upon natural as well as artificial processes.

First, as regards the formation of aqueous deposits, it is obvious that it is but very rarely that they can be deposited otherwise than in a flocculated condition. As a rule, the water cannot assort in accordance with their individual hydraulic value, any sediments below that of 8^{mm} per second, or about $0{\cdot}2^{mm}$ diameter; and the complex particles thus formed are deposited along with single ones of much higher hydraulic value. The deposition of such an intractable substance as "pure clay" would be, as well as that of any one sediment of very limited range as to size or hydraulic value, is thus practically impossible, save under very exceptional circumstances. Were this otherwise, the local occurrence of soils and rocks of extreme character, both as to chemical and mechanical composition, would be much more frequent than is actually the case.

The fact that, as a consequence of known processes in the formation of aqueous deposits, the latter must be considered as constituted of compound spheroid granules, at once determines the view of Tyndall regarding the cause of slaty cleavage by compression, to be the true one, as against that of Sorby. Yet Tyndall, while assuming such structure to exist, fails to assign any definite cause, outside of crystalline masses. It would be interesting to make the counter-test, and prove that when (as can readily be done) the flocculated structure is destroyed

previous to the application of pressure, slaty structure does not result, either at all, or in as marked a degree. I hope to make this experiment before long.

The destruction of the floccules is effected by what is known in the arts as the tamping or "puddling" of earth or clay. It is the result of violent agitation with water, or of kneading, boiling, or finally, to a certain extent, of freezing. All these agencies are employed by the workers in clay for the purpose of increasing plasticity, which depends essentially upon the finest possible condition of the material to be worked: for when it is in a flocculent or granular condition, it assumes more or less the properties of sand.

The latter fact can be most strikingly shown in the following manner: Let any clay or clay soil be worked into a plastic paste with water and then dried; the result will be a mass of almost stony hardness. Add to the same paste about 0·5 per cent of caustic lime, which substance (as shown by Schlœsing and myself) possesses in an eminent degree the property of coagulating clay into floccules. The diminution of plasticity will be obvious at once, even in the wet condition; and upon drying, the mass will fall into a pile of crumbs upon a mere touch, or dropping it on the floor—a striking lecture experiment, as well as a convincing illustration of the effect of liming upon clay soils, in rendering them "warmer" and more readily tilled. I found that bubbling carbonic acid gas through a magma of the limed clay for twenty-four hours, when all alkaline reaction had disappeared, had failed to restore the plasticity even when, after drying, the carbonate solution had been destroyed. This agrees with the experience of farmers that the "lightening" effect of a liming continues for years to be very manifest, and is never entirely lost. It is well known that marling produces similar but weaker effects; and the same can be observed wherever one and the same clay soil is partially subject to washings from limestone hills, or to the admixture of underlying calcareous strata. Schlœsing's experiments on the efficacy of lime water in coagulating clay water, show its effect to exceed greatly that of any other calcium compounds; and so far as my experiments go, I find those of no other element approaching, in this respect, those of calcium. In this connection Dr. John LeConte's remarks on the exceptional transparency of calcareous waters in Florida (Proc. Am. Assoc. Adv. Sci., 1860, p. 33) should be called to mind.

Were any farther proof needed regarding the nature of the effects of tillage upon soils, this lime experiment would supply it. *It is the loosely flocculated aggregation of the soil particles which constitutes good tilth*, as against the partially "tamped" condition which results from the mechanical action of rains

and (in silt soils) of drying. Tillage acts not merely, as Professor Johnson says (On some Reasons for Tillage, Rep. Sec'y, Conn. Board of Agr., 1877–8): "by lifting up masses of soil, turning them and letting them fall so that the close contact produced by rest (?) and moving water is broken, and the grains of sand and the minute aggregations of loam are brought into new positions with regard to each other, and to greater distances from each other." These "new positions" and "greater distances" are brought about and maintained by the phenomenon of flocculation, by the formation of compound crumbs ("ackerkrume"), which can be readily seen and compared with the partially "tamped" soil on the land side, in any furrow made in well-tilled cultivated soil; and they relate to each other, both as to bulk and permeability, in mass, somewhat as powdered lead would to bird-shot. If this be the correct view, mere *rest* cannot, in general, produce any compacting influence on soils, such as is claimed (loc. cit.) by Professor Johnson; and I think the proof that it is innocent of any such action is found in the case of any virgin soil in forests, where the protecting covering of leaves assures the most absolute rest to the soil for centuries, and where, nevertheless, it is always in such tilth as our best-tilled fields might envy.

There is one class of soils, nevertheless, in which "rest" may be said to produce compacting, to wit: those consisting of siliceous silt with not clay enough to maintain them in position after drying; so that the loose arches of the floccules collapse by their own weight, or by the least shocks. This happens not unfrequently in river-sediment soils; and the curious result is that they are injured by plowing when very *dry*, in the same way that clayey soils are when plowed too *wet*. In both cases the effect is to destroy the floccules and produce a single-grain or tamped structure, which it requires several seasons' tillage to rectify. In the case of "beating" rains, the hardening effect upon the surface is of course due to the mechanical destruction of the floccules, followed by the infiltration with "clay water," which, in drying, acts as a hardening cement.

In some soils the point of maximum cohesion of the floccules, and consequently the tendency to maximum flocculation, lies between very narrow limits; as the farmer says, they have to be plowed when "just right," or not at all, on pains of injuring their tilth for years. I find among these not only the clay soils proper, but another class consisting of uniformly fine silts (chiefly 1^{mm} hydraulic value, and below) with but very little clay; which, when wet, are said to "work like putty," and when a very little too dry remain cloddy. Those in which the tillage may be done within a wide range of condition as to moisture, are those containing a great variety of sediments of

all sizes; colluvial rather than alluvial, and usually called "loams." The exact conditions of maximum flocculability, however, and the influence exerted thereon by each of the several sediments and their combinations, still remain to be determined; and the task is not a light one.

It is known that the longer a soil has been maintained in perfect tilth, the more difficult it is to "puddle" it by wet plowing. This is doubtless owing to the gradual cementation of the floccules by the soil water; which fixes them, as it were, more or less permanently.

As to the action of frost on soils, it is clear that as the water-menisci within the floccules freeze, the soil must be reduced to its ultimate mechanical elements, held apart by the ice crystals. It is thus readily intelligible why plowing too wet, *after a freeze*, is so much more injurious to tilth than when done after only a rain. But when the thawed soil is allowed first to assume its proper moisture-condition, the newly-formed floccules are of course looser than ever; and in clay and loam soils the most perfect tilth is the result, while *clayless* ones are measurably "*puddled*" *by frost.*

The "ripening" of potter's clay by freezing, boiling, alternate wetting and drying, or finally by thorough working, is obviously based on the same process of "atomization." Not only do these processes destroy the mechanically formed aggregates, but boiling at least, and probably all the rest, serve to reduce the kaolinite ingredient of soils and clays to the amorphous, plastic, diffusible modification. As to boiling, I have succeeded in rendering diffusible and very plastic, the seventy-five per cent of clayey ingredient in a white, chalky pipe clay, resembling kaolin, and which naturally is but very slightly plastic, by boiling for about eighty-five hours. In this case the clay water showed, even after a week's subsidence, a distinctly dotted structure under a power of five hundred diameters, instead of the non-resolvable cloud exhibited by the water from naturally plastic clays. Since lithological evidence proves conclusively that mere soaking in water does not produce the transformation, unless accompanied by mechanical agencies: we can hardly escape the conclusion that the difference between the plastic and non-plastic kaolinite is merely mechanical, and not properly comparable to that between quartz and chalcedony.

In regard to the action of alkaline carbonates in preventing flocculation, and thus rendering tillage difficult or impossible, many of the "alkali soils" of California supply striking examples. The name is popularly applied, almost indiscriminately, to any soil containing such excess of soluble salts of any kind, as to become apparent by efflorescence. Among these, however, the soils impregnated with alkaline carbonates may

generally be recognized by their extreme compactness, and refractoriness under tillage; and by the fact that they usually form "low spots" in the general surface of non-alkaline land, where turbid clay water, dark with dissolved humus, will lie for weeks after the higher land appears dry. I give below the results of the comparative mechanical analysis of two samples of soil, taken within twelve feet of each other, on an alkaline tract in the neighborhood of Stockton, Cal. One of these represents a fertile "ridge" soil, in excellent tilth, lying (naturally) about eighteen inches higher than the soil of the adjacent alkali tract, in which the same soils alternate on hillocks and in depressions, so as to render it impossible to cultivate one without the other. The alkali soil had been plowed, cross-plowed, rolled and harrowed, until the harrow produced no further effect; and the result was a seed-bed of soil clods ranging from the size of a pea to that of a billiard ball, but no tilth. At the same time, portions of the "ridge" soil so treated were reduced to an ashy condition of tilth. Inspection seemed to show that the two soils differ but little in mechanical composition; not nearly enough to account for such difference of tilling qualities. The analyses resulted thus:

STOCKTON SOILS.

				Non-alkaline.	Alkaline.
Clay,				20·8	24·6
Sediment	< 0·25^{mm}	hydraulic	value	32·0	26·1
"	0·25	"	"	3·3	3·3
"	0·50	"	"	6·6	9·4
"	1·0	"	"	5·6	6·2
"	2·0	"	"	7·3	6·2
"	4·0	"	"	7·5	5·4
"	8·0	"	"	5·7	4·8
"	16·0	"	"	4·8	4·7
"	32·0	"	"	1·5	5·9
"	64·0	"	"	1·2	1·1
				96·4	97·7

The only material difference in the mechanical composition of these two soils is in the relative amounts of clay and finest sediment; and this is perhaps more apparent than real, inasmuch as the disintegrating action of the alkali has doubtless been instrumental in bringing out as "clay" in the alkaline soil, a portion of what in the other has remained behind in the finest sediment. Still, as the alkali soil lies close to, and lower than, the tillable soil, and receives its washings, it should contain somewhat more clay than the latter. Taking the results as they are, however, it is clear that the mechanical composition of the alkali soil gives no clue to the cause of its be-

havior under tillage, since a soil containing no more than 24·6 per cent of clay, with all sediments above the finest so evenly distributed, should be classed simply as a "clayey loam."

The dark brown soil-extract, leached from the alkali soil, amounted, after ignition and treatment with carbonic acid, to 0·251 per cent. Of this amount, 0·158 was again soluble, 0·093 remaining behind as earthy salts, etc. The soluble part was constituted thus:

Carbonate of sodium	52·74
Chlorid of sodium	38·08
Sulphate of sodium	13·26
Tri-sodic phosphate	1·83
	100·91

The insoluble part of the aqueous extract gave:

Carbonate of calcium	14·02
Tri-calcic phosphate	5·37
Tri-magnesic phosphate	5·77
Silica (soluble in Na_2CO_3)	24·37
Iron oxides, alumina, and some clay (by difference)	50·47
	100.00

It will be observed that notwithstanding the presence of considerable amounts of neutral sodium and calcium salts, that of about 0·08 per cent of carbonate of sodium was sufficient to render the soil practically untillable. In this case, as well as in that of a very large amount of similar "alkali land" in the state, the application of a sufficient amount of gypsum to decompose the alkaline carbonate, produces a surprising change; which, on the small scale, can be perceived at once, but in the field, naturally requires a season's tillage to become effective as to tilth. It at once prevents, of course, the injury to seeds and growing plants arising from the corrosive action of the alkaline carbonate, which helps to render them unprofitable for culture.

My suggestion of the use of gypsum on soils of this character, not containing enough of soluble salts to render their presence objectionable when neutral, has already been tested in practice with the most satisfactory results. It will be seen that among the soluble salts is a comparatively very large amount of alkaline and earthy phosphates, which (in accordance with Grandeau's researches) follow the humus acids into solution even when the solvent is alkaline. Were the soil to be thorough-drained (as has sometimes been done in order to relieve the excess of salts) these phosphates would be carried off in the drain water. The use of gypsum prevents this loss by rendering the phosphates insoluble together with the humus, albeit in such a finely

divided state as to remain perfectly available to vegetation. The double benefit thus gained manifests itself in the exuberant fertility of the soils so treated. I shall hereafter describe more in detail the very singular features of the California alkali soils, but mention the present as a conspicuous example of the benefits to be derived from an intelligent, direct investigation of soils.

Among the many other cases in which the phenomenon of flocculation comes into play in technical practice, I only mention that of ore extraction by means of solvents. Were the solvent liquid poured on the finely pulverized dry ore, it would permeate it with extreme slowness, and the subsequent washing-out of the solution would be tedious in the extreme. It is therefore the universal practice with all fine ores to dampen them first with an amount of water varying according to the nature and fineness of the ore, and then by stirring or working over, to flocculate them, or, in technical language, to render them "woolly." Care is then taken to prevent the inflow of water from "puddling" the charge by direct impact, and all stirring or concussion whereby the flocculated arrangement of the particles might be broken down, is carefully avoided.

Similarly, of course, the leaching-out the soluble salts from a true "alkali soil" by the ordinary process of percolation, is almost impossible in consequence of the entire absence of flocculation; and can only be accomplished by a long series of decantations followed by evaporation to dryness, extraction of the residue by quiet upward diffusion, and syphoning-off. Even thus, a final correction for clay that has defied both filtration and subsidence, must almost always be made.

ART. XXV.—*Remarks on the Jura-Trias of Western North America;* by C. A. WHITE, Paleontologist to the U. S. Geological Survey.

THE fact is generally known that among North American rocks neither the Jurassic nor Triassic period are any where so well represented as they are in certain parts of Europe. Indeed, until within a few years past it has not been claimed as demonstrated by the presence of characteristic types of fossils that any strata of the western territories of the United States are exactly equivalent with any Triassic strata of Europe, although a certain series, distributed over a large portion of that great western region has been recognized as Jurassic; and also a certain sandstone formation, usually of a red or reddish color, generally present beneath that Jurassic series, has been

very generally referred to the Trias. It is my present purpose to offer some remarks upon these western Mesozoic formations, but not now to consider those strata which are commonly referred to the Triassic period in Connecticut and certain Atlantic States farther south.

Some modification of the views concerning these western formations that have been just indicated was produced, but the elucidation of the question of actual synchronism of these two groups with European strata hardly advanced by the following circumstances. The sandstone formation before referred to, which, from its prevalent color has been often designated as the "Red Beds," had been generally regarded as unfossiliferous, except that some imperfect vertebrate remains and an abundance of silicified wood have been found in its strata; but Dr. Hayden, in 1869, published* the statement that he had found unmistakable Jurassic fossils near the base of this group which until then had been referred to the Trias almost without question. Also, in 1874, Mr. E. E. Howell, then in charge of one of Professor Powell's parties, collected in southern Utah, from the lower portion of the formation in question, several species of invertebrate fossils, some of which, at least, I regard as specifically identical with well-known species which, until then, had been found only in the unquestioned Jurassic strata of the western territories.† As far as this paleontological evidence extended, it seemed to indicate that all the Mesozoic strata of that great region east of western Nevada, and beneath the Dakota group of the Cretaceous series belong to one and the same epoch. The impression however still prevailing that the Triassic period, at least in part, is really represented by the Red Beds, the term Jura-Trias began to be used to designate collectively that formation together with the Jurassic strata above it. This collective designation seems to have met with general approval and, although our knowledge concerning these strata and their equivalents is increasing, it seems advisable to continue it until further investigation shall enable us to fix upon some definite horizon for their separation.

None of the foregoing remarks are intended to apply to the great development of Upper Triassic strata which, as is well known, exist near the western border of the continent. The existence of these strata was first made known in 1864, by the publication of volume I, Paleontology of California; and they were still further described in the lately published volumes of the United States Geological Exploration, 40th Parallel. In both

* See page 11, Geol. Rep. Expl. Yellowstone and Missouri Rivers. By F. V. Hayden, under the direction of Capt. W. F. Raynolds, Corps of Engineers U. S. A. Made in 1860 and published in 1869. 8vo, pamphlet.

† See White's paleontological chapter in Powell's Report on the Geology of the Uinta Mountains, pages 80 and 87.

of these works the contained fossils are fully illustrated and described, and the strata are, by those fossils, referred to the horizon of the St. Cassian, Aussee, and Hallstadt deposits of Europe. Professor Whitney says in the first work just cited: "This great Triassic belt of the Pacific coast has been most fully explored by the survey in the latitude of 40°, and over a width east and west of nearly four degrees of longitude (117° to 121°) . . . . But sufficient paleontological evidence has been obtained to enable us to state that this formation extends from Mexico to British Columbia, and that it occupies a vast area, although much broken up, interrupted by eruptive rocks, and covered in many places by heavy accumulations of volcanic materials."

No fossils, however, as already stated, or at least no invertebrates, were known to exist in any North American strata east of that region which could be confidently referred to the Triassic period, until the autumn of 1877, when a collection was brought in by one of the parties of the United States Geological Survey of the Territories, in charge of Dr. F. V. Hayden, which was found to contain molluscan types that are plainly Triassic. The collection referred to was made by Dr. A. C. Peale, field geologist, from two or three localities in southeastern Idaho, and which, with the other collections of the survey, came into my hands for study. They have, however, only lately received investigation, and their preliminary publication will soon be made in Part I, vol. v, Bull. U. S. Geol. Sur. Terr.

The fossils which have been obtained at numerous localities in different parts of the western territories and referred with little or no question to the Jurassic period are well known; such, for example, as *Pentacrinus asteriscus* Meek and Hayden, *Belemnites densus* M. & H., *Camptonectes bellistriatus* M. & H., *Eumicrotis curta* Hall, *Ostrea strigilecula* White, &c. Wherever in that region a collection of Jurassic (or as they have come to be commonly designated, Jura-Trias) fossils has been made, some one or more of these species has generally been found among them; and the new forms, whenever they have been discovered, have not generally been in excess of those which were previously known, thus suggesting the discovery of no separate horizons. Almost the contrary, however, is the case with the collection from the localities here referred to in southeastern Idaho, because, out of the list of the species obtained there and presently to be mentioned, only three were previously known, and only one (*Eumicrotis curta*) has ever been found associated with well-known Jurassic fossils.

At the principal one of these localities, which is about sixty-five miles north of the southern, and eighteen miles west of the eastern, boundary of Idaho, Dr. Peale found the Jura-Trias

strata in question to be about 3,000 feet in thickness of alternating limestones, sandy shales and sandstones, with about 1,800 feet of Carboniferous strata beneath them, and with which they are apparently strictly conformable. He found in that immediate vicinity no exposures of the Red Beds, which have so generally been referred to the Trias, but he recognized them at some localities only a few miles away; and he is confident that they occupy a position immediately above the uppermost strata of the series exposed at the locality here especially referred to. It thus appears that this fossiliferous series, containing true Triassic types, occupies a position beneath the comparatively unfossiliferous Red Beds which were so long supposed to be the only representatives of the Trias in that great region; and that they separate the former from the well-known Jurassic series. The fossils of this lower series were found at different horizons within the thickness of 3,000 feet exposed at the locality mentioned, the following being a list of them:

1. *Terebratula semisimplex* White; 2. *T. augusta* Hall & Whitfield?; 3. *Aviculopecten Idahoensis* Meek; 4. *A. Pealei* W.; 5. *A. altus* W.; 6. *Eumicrotis curta* Hall; 7. *Meekoceras aplanatum* W.; 8. *M. Mushbachanus* W.; 9. *M. gracilitatus* W.; 10. *Arcestes? cirratus* W.; 11. *A.?*———?

All except Nos. 2, 3 and 6 of this list are new forms, and they have hitherto been found only within that limited area in southeastern Idaho. No. 2, which is referred by its authors to the Jurassic of Nevada, is doubtfully identified as above. The type specimens of No. 3 were obtained from another locality in southeastern Idaho several years ago, and doubtless from the same horizon as that of the foregoing list. No. 6 is a common fossil and has by different geologists been reported from numerous and widely separated Jurassic localities in the west. Indeed it is at present, by this species alone, that the collection of the foregoing list is connected with the well-recognized Jurassic of that great region.

The Brachiopods and Conchifers of this collection would not have attracted particular attention under the circumstances, but the Cephalopods present types which had never been found in American Jurassic strata, but are clearly Triassic; one of which was a new generic form. Having recognized the unique character of this collection, the Triassic type of the Cephalopods, and the fact that although closely related to typical *Ceratites*, one group especially presented some important generic modifications, concerning which I desired the opinion of Professor Alpheus Hyatt, whose excellent and exhaustive labors upon this class of fossils are so well known. I therefore sent them to him for examination, and of the two, or perhaps three, generic forms which they embrace he describes one as new,

under the name of *Meekoceras*, and refers the others, doubtfully, because of the imperfection of the specimens, to *Arcestes* Suess.

Now comes an interesting and, in view of the recognized St. Cassian age of the Nevada Trias before referred to, a somewhat unexpected fact. The types of these Cephalopods, as distinctly stated by Professor Hyatt, have close affinities with those of the Muschelkalk or Middle Trias of Europe, and not with those of the St. Cassian, Aussee, and Hallstadt deposits of the European Upper Trias. This fact, however, need not excite especial surprise when it is remembered that the southeastern Idaho locality is about four hundred miles eastward from that of the Nevada Trias; and, furthermore, that the positions which they occupy are respectively adjacent to east and west borders of an extensive area which was above the level of the sea during the whole of Mesozoic time; and which extended from the southern to the northern portion of North America. The deposits of the two regions were therefore never directly continuous, and probably no physical connection of their strata respectively, need be sought for.

With this discovery of a true Triassic fauna east of that long continental area of Mesozoic time, new interest is given to the study of the earlier Mesozoic rocks of the western territories. There are such indications of an intimate faunal relationship between these Triassic strata of southeastern Idaho and the true Jurassic strata of that great western region, that I still prefer to hold to the provisional designation of Jura-Trias for the whole of them. But in view of the great development of the first mentioned strata, although now known only in a limited area, and of the distinctively Triassic facies of its types, especially the Cephalopods, it seems not improbable that we may yet find a physical plane for their separation as Jurassic and Triassic groups respectively.

ART. XXVI.—*On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules;* by WILLIAM CROOKES, F.R.S., V.P.C.S.*

Induction Spark through Rarefied Gases.—Dark Space round the Negative Pole.—The author has examined the dark space which appears round the negative pole of an ordinary vacuum-tube when the spark from an induction-coil is passed through it. He describes many experiments with different kinds of poles, a varying intensity of spark, and different gases, and arrives at the following propositions:—

* Abstract of a paper read before the Royal Society, Dec. 5, 1878. (Phil. Mag., Jan., 1879.)

Illumination of Lines of Molecular Pressure.—a. Setting up an intense molecular vibration in a disk of metal by electrical means excites a molecular disturbance which affects the surface of the disk and the surrounding gas. With a dense gas the disturbance extends a short distance only from the metal; but as rarefaction continues, the layer of molecular disturbance increases in thickness. In air at a pressure of 0·078 millim. this molecular disturbance extends for at least 8 millims. from the surface of the disk, forming an oblate spheroid around it.

b. The diameter of this dark space varies with the exhaustion, with the kind of gas in which it is produced, with the temperature of the negative pole, and, in a slight degree, with the intensity of the spark. For equal degrees of exhaustion it is greatest in hydrogen and least in carbonic acid, as compared with air.

c. The shape and size of this dark space do not vary with the distance separating the poles, nor (or only very slightly) with alteration of battery-power, nor with intensity of spark. When the power is great the brilliancy of the unoccupied parts of the tube overpowers the dark space, rendering it difficult of observation; but, on careful scrutiny, it may still be seen unchanged in size; nor does it alter even when, with a very faint spark, it is scarcely visible. On still further reduction of the power it fades entirely away, but without change of form.

The author describes numerous experiments, devised to ascertain if this visible layer of molecular disturbance is identical with the invisible layer of molecular pressure or stress, the investigation of which has occupied him for some years.

The Electrical Radiometer.—One of these experiments is as follows:—An ordinary radiometer is made, with aluminium disks for vanes, each disk coated with a film of mica. The fly is supported by a hard steel cup instead of a glass cup; and the needle-point on which it works is connected by means of a wire with a platinum terminal sealed into the glass; at the top of the radiometer-bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction-coil, the movable fly being made the negative pole.

Passing over the phenomena observed at low exhaustions, the author finds that, when connected with the coil, a halo of a velvety violet light forms on the metallic side of the vanes, the mica side remaining dark throughout these experiments. As the pressure diminishes a dark space is seen to separate the violet halo from the metal. At a pressure of half a millim. this dark space extends to the glass, and positive rotation commences.

On continuing the exhaustion, the dark space further widens out and appears to flatten itself against the glass, and the rotation becomes very rapid.

When aluminium cups are used for the vanes instead of disks backed with mica, similar appearances are seen. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. On further exhaustion, the dark space on the convex side touches the glass, when positive rotation commences, becoming very rapid as the dark space further increases in size and ultimately flattens against the glass.

Convergence of Molecular Rays to a Focus.—The subject next investigated is the convergence of the lines of force to a focus, as observed with the aluminium cup. As this could not be accomplished during rapid rotation, an instrument was made having the cup-shaped negative pole fixed instead of movable. On exhaustion, the convergence of the lines of force to a focus at the concave side was well observed. When the dark space is very much larger than the cup, it forms an irregular ellipsoid, drawn in toward the focal point. Inside the luminous boundary a focus of dark violet light can be seen converging, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space—the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere.

Green Phosphorescent Light of Molecular Impact.—At very high exhaustions the dark space becomes so large that it fills the tube. Careful scrutiny still shows the presence of the dark violet focus; and the part of the glass on which fall the rays diverging from this focus shows a sharply defined spot of greenish-yellow light. On still further exhaustion, and especially if the cup is made positive, the whole bulb becomes beautifully illuminated with greenish-yellow phosphorescent light.

This greenish-yellow phosphorescence, characteristic of high exhaustions, is frequently spoken of in the paper. It must be remembered, however, that the particular color is due to the special kind of soft German glass used. Other kinds of glass produce a different color. The phosphorescence takes place only under the influence of the negative pole. At an exhaustion of 4 M* no light other than this is seen in the apparatus. At 0·9 M the phosphorescence is about at its maximum. When the exhaustion reaches 0·15 M, the spark has a difficulty in passing, and the green light appears occasionally in flashes only. At 0·06 M the vacuum is almost non-conductive; and a spark can be forced through only by increasing

* M signifies the millionths of an atmosphere.

the intensity of the coil and well insulating the tube and wires leading to it. Beyond that exhaustion nothing has been observed.

Focus of Molecular Force.—In an apparatus specially constructed for observing the position of the focus the author found that the focal point of the green phosphorescent light was at the center of curvature, showing that the molecules by which it is produced are projected in a direction normal to the surface of the pole. Before reaching the heat exhaustion for the green light, another focus of blue-violet light is observed; this varies in position, getting further from the pole as the exhaustion increases. In the apparatus described, at an exhaustion of 19·3 M, these two foci are seen simultaneously, the green being at the center of the curvature, while the blue focus is at nearly twice the distance.

Nature of the Green Phosphorescent Light.—The author adduces the following characteristics of the green phosphorescent light, as distinguishing it from the ordinary light observed in vacuum-tubes at low exhaustions:

a. The green focus cannot be seen in the space of the tube, but where the projected beam strikes the glass only.

b. The position of the positive pole in the tube makes scarcely any difference in the direction and intensity of the lines of force which produce the green light. The positive pole may be placed in the tube either at the extremity opposite the negative pole, or below it, or by its side.

c. The spectrum of the green light is a continuous one, most of the red and the higher blue rays being absent; while the spectrum of the light observed in the tube at lower exhaustions is characteristic of the residual gas. No difference can be detected by spectrum-examination in the green light, whether the residual gas be nitrogen, hydrogen, or carbonic acid.

d. The green phosphorescence commences at a different exhaustion in different gases.

e. The viscosity of a gas is almost as persistent a characteristic of its individuality as its spectrum. The author refers to a preliminary note and a diagram* of the variation of viscosity of air, hydrogen, and other gases at exhaustions between 240 M and 0·1 M. From these and other unpublished results, the author finds that the viscosity of a gas undergoes very little diminution between atmospheric pressure and an exhaustion at which the green phosphorescence could be detected. When, however, the spectral and other characteristics of the gas begin to disappear, the viscosity also commences to decline; and at an exhaustion at which the green phosphorescence is most brilliant the viscosity has rapidly sunk to an insignificant amount.

* Proc. Roy. Soc., Nov. 16, 1876, vol. xxv, p. 305.

f. The rays exciting green phosphorescence will not turn a corner in the slightest degree, but radiate from the negative pole in straight lines, casting strong and sharply defined shadows from objects which happen to be in their path. On the other hand, the ordinary luminescence of vacuum-tubes will travel hither and thither along any number of curves and angles.

Projection of Molecular Shadows.—The author next examines the phenomena of shadows cast by the green light. The best and sharpest shadows are cast by flat disks and not by narrow-pointed poles; no green light whatever is seen in the shadow itself, no matter how thin, or whatever may be the substance from which it is thrown.

From these and other experiments, fully described in the paper, he ventures to advance the theory that the induction-spark actually illuminates the lines of molecular pressure caused by the electrical excitement of the negative pole. The thickness of the dark space is the measure of the mean length of the path between successive collisions of the molecules. The extra velocity with which the molecules rebound from the excited negative pole keep back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the collisions.

When the exhaustion is sufficiently high for the mean length of path between successive collisions to be greater than the distance between the fly and the glass, the swiftly moving rebounding molecules spend their force, in part or in whole, on the sides of the vessel, and the production of light is the consequence of this sudden arrest of velocity. The light actually proceeds from the glass, and is caused by fluorescence or phosphorescence on its surface. No light is produced by a mica or quartz screen; and the more fluorescent the material the better the luminosity. Here the consideration arises that the greenish-yellow light is an effect of the direct impact of the molecules in the same electrical state on the surface of the glass. The shadows are not optical, but are molecular shadows revealed only by an ordinary illuminating effect; this is proved by the sharpness of the shadows when projected from a wide pole.

Phosphorescence of Thin Films.—An experiment is next described in which a film of uranium glass, sufficiently thin to show colors of thin plates, is placed in front of a thick plate of the same glass, the whole being closed in a tube with terminals and exhausted to a few millionths of an atmosphere. Of this the following observations are recorded:—

a. The uranium film, being next to the negative pole, casts a strong shadow on the plate.

b. On making contact with the coil, the thin film flashes out suddenly all over its surface with a yellowish phosphorescence, which, however, instantly disappears. The uncovered part of the plate does not become phosphorescent quite suddenly, but the phosphorescence is permanent as long as the coil is kept at work.

c. With an exceedingly faint spark the film remains more luminous than the plate; but on intensifying the spark, the luminosity of the film sinks, and that of the uncovered part of the plate increases.

d. If a single intense spark be suddenly sent though the tube, the film becomes very luminous, while the plate remains dark.

These experiments are conclusive against the phosphorescence being an effect of the radiation of the phosphorogenic ultra-violet light from a thin layer of arrested molecules at the surface of the glass; for were this the case, the film could under no circumstances be superior to the plate.

The momentary phosphorescence and rapid fading of the film prove more than this. The molecular bombardment is too much for the thin film. It responds thereto at first, but immediately gets heated by the impacts, and then ceases to be luminous. The plate, however, being thick, bears the hammering without growing hot enough to lose its power of phosphorescing.

Mechanical Action of Projected Molecules.—When the coil was first turned on, the thin film was driven back at the moment of becoming phosphorescent, showing that an actual material blow had been given by the molecules. Experiments are next described in which this mechanical action is rendered more evident. A small rotating fly, capable of being moved about in any part of an exhausted bulb, is used as an indicator; and by appropriate means the molecular shadow of an aluminium plate is projected along the bulb. Whether entirely in or entirely out of the shadow, the indicator scarcely moves; but when immersed so that one-half is exposed to molecular impact the fly rotates with extreme velocity.

Magnetic Deflection of Lines of Molecular Force.—With this apparatus another phenomenon was investigated. It is found that the stream of molecules whose impact on the glass occasions evolution of light is very sensitive to magnetic influence; and by bringing one pole of an electromagnet, or even of a small permanent magnet, near, the shadow can be twisted to the right or to the left.

When the little indicator was placed entirely within the molecular shadow no movement was produced. As soon, however, as an adjacent electromagnet was excited, the shadow was

twisted half off the indicator, which immediately rotated with great speed.

The Trajectory of Molecules.—The amount of deflection of the stream of molecules forming a shadow is in proportion to the magnetic power employed.

The trajectory of the molecules forming the shadow is curved when under the magnetic influence; the action of the magnet is to twist the trajectory of the molecules round in a direction at an angle to the free path, and to a greater extent as they are nearer the magnet, the direction of twist being that of the electric current passing around the electromagnet.

Laws of Magnetic Deflection.—An apparatus was constructed so that the deflection of a spot of light was used instead of that of a shadow, a horseshoe magnet being placed underneath the negative pole to deflect the trajectory. The action of the north pole being to give the line of molecules a spiral twist one way, and that of the south pole being to twist in the other way the two poles side by side compel the line to move in a straight line up or down, along a plane at right angles to the plane of the magnet and a line joining its poles.

The ray of molecules does not appear to obey Ampère's law, as it would were it a perfectly flexible conductor, joining the negative and the positive pole. The molecules are projected from the negative: but the position of the positive pole—whether in front, at the side, or even behind the negative pole—has no influence on their subsequent behavior, either in producing phosphorescence, or mechanical effects, or in their magnetic deflection. The magnet gives their line of path a spiral twist, greater or less according to its power, but diminishing as the molecules get further off.

Numerous experiments were tried in this apparatus with different gases, and with the magnet in and out of position.

Working with exhausted air it was found that the spot of green phosphorescence on the screen is visible at an exhaustion of 102·6 millim., when the mean free path of the molecules, measured by the thickness of the dark space round the negative pole, is only 12 millims. Hence it follows that a number of molecules sufficient to excite green phosphorescence on the screen are projected the whole distance from the pole to the screen, or 102 millims., without being stopped by collisions.

Alteration of Molecular Velocity.—If we suppose the magnet to be permanently in position, and thus to exert a uniform downward pull on the molecules, we perceive that the trajectory is much curved at low exhaustions, and gets flatter as the exhaustion increases. A flatter trajectory corresponds to a higher velocity. This may arise from one of two conditions: either the initial impulse given by the negative pole is stronger

or the resisting medium is rarer. The latter is probably the true one. The molecules which produce the green phosphorescence must be looked upon as in a state differing from those arrested by frequent collisions. The latter impede the velocity of the free molecules and allow longer time for magnetism to act on them; for, although the deflecting force of magnetism might be expected to increase with the velocity of the molecules, Prof. Stokes has pointed out that it would have to increase as the square of the velocity, in order that the deflection should be as great at high as at low velocities.

Comparing the free molecules to cannon-balls, the magnetic pull to the earth's gravitation, and the electrical excitation of the negative pole to the explosion of the powder in the gun, the trajectory will be flat when no gravitation acts, and curved when under the influence of gravitation. It is also much curved when the ball passes through a dense resisting medium; it is less curved when the resisting medium gets rarer; and, as already shown, intensifying the induction spark, equivalent to increasing the charge of powder, gives greater initial velocity, and therefore flattens the trajectory. The parallelism is still closer if we compare the evolution of light seen when the shot strikes the target, with the phosphorescence on the glass screen from molecular impacts.

Focus of Heat of Molecular Impact.—The author finally describes an apparatus in which he shows that great heat is evolved when the concentrated focus of rays from a nearly hemispherical aluminium cup is deflected sideways, to the walls of the glass tube, by a magnet. By using a somewhat larger hemisphere, and allowing the negative focus to fall on a strip of platinum-foil, the heat rises to the melting-point of platinum.

An Ultra-gaseous state of Matter.—The paper concludes with some theoretical speculations on the state in which the matter exists in these highly-exhausted vessels. The modern idea of the gaseous state is based upon the supposition that a given space contains millions and millions of molecules in rapid movement in all directions, each having millions of encounters in a second. In such a case, the length of the mean free path of the molecules is exceedingly small as compared with the dimensions of the vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed. But by great rarefaction the free path is made so long that the hits in a given time may be disregarded in comparison with the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the vessel, the properties which constitute gaseity are reduced to a minimum, and the matter becomes

exalted to an ultra-gaseous state, in which the very decided but hitherto masked properties now under investigation come into play.

Rays of Molecular Light.—In speaking of a ray of molecular light the author has been guided more by a desire for conciseness of expression than by a wish to advance a novel theory. But he believes that the comparison, under these special circumstances, is strictly correct, and that he is as well entitled to speak of a ray of molecular or emissive light when its presence is detected only by the light evolved when it falls on a suitable screen, as he is to speak of a sunbeam in a darkened room as a ray of vibratory or ordinary light when its presence is to be seen only by interposing an opaque body in its path. In each case the invisible line of force is spoken of as a ray of light; and if custom has sanctioned this as applied to the undulatory theory, it cannot be wrong to apply the expression to the emissive light. The term emissive light, must, however, be restricted to the rays between the negative pole and the luminous screen; the light by which the eye then sees the screen is, of course, undulatory.

The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line—but where we can never enter, and in which we must be content to observe and experiment from the outside.

ART. XXVII.—*On the Chemical Composition of Triphylite;* by SAMUEL L. PENFIELD, Ph.B., Assistant in the Sheffield Laboratory.—*Contributions from the Sheffield Laboratory of Yale College*, No. LIII.

IN June, 1877, I published* an analysis of triphylite from Grafton, New Hampshire, and showed that the composition of that variety of the species conformed to the general formula $\overset{II}{R_3}P_2O_8+\overset{I}{R_3}PO_4$. I am now able to add analyses of the same mineral from Norwich, Massachusetts, and from Bodenmais, Bavaria, and also of the allied manganese-lithium phosphate, lithiophilite, from Branchville, Conn.; the results of these analyses remove all doubt in regard to the true formula belonging to this group.

The specimen from Norwich, Massachusetts, I received from Professor Brush. He had previously identified it as† forming the nucleus of a large crystal of the well known pseudomorphous black phosphate of iron, manganese and lithium, which

* This Journal, III, xiii, 426, June, 1877. † Ibid., II, xxxiv, 402, Nov. 1862.

occurs at that locality associated with spodumene. This unaltered triphylite is very rare, but the specimen, a portion of which was used for analysis, was bright and lustrous, with the distinct cleavages, and all the physical characters of the original mineral. The following analyses give additional proof of its being quite free from alteration.

(1.) Triphylite from Norwich, Mass.; color greyish green, specific gravity =3·534.

	I.	II.	Mean.		Atomic relation.	
P_2O_5	44·72	44·80	44·76	P	·630	
FeO	26·40	26·40	26·40	Fe	·366	
MnO	17·87	17·80	17·84	Mn	·251	=·633
CaO	·16	·33	·24	Ca	·004	
MgO	·49	·45	·47	Mg	·012	
Li_2O	9·37	9·34	9·36	Li	·622	=·632
Na_2O	·32	·38	·35	Na	·010	
H_2O	·53	·30	·42			
	99·86	99·80	99·84	Li_2	·311	=·316
				Na_2	·005	

The analyses of the Bavarian triphylite which have been published do not agree very satisfactorily, and have left some doubt in regard to the true composition of the mineral. With a view to settling this point I have also analyzed an authentic specimen from Bodenmais, Bavaria. This was furnished to me by Professor Brush; he had received it from Dr. Hugo Müller, who had himself collected it at the locality. It was quite pure, and entirely free from any traces of alteration. The results of the analysis are given below; it will be seen that it has afforded an exceptionally large percentage of lithia.

(2.) Triphylite from Bodenmais; color light blue, specific gravity =3·549.

	I.	II.	Mean.		Atomic relation.	
P_2O_5	43·16	43·19	43·18	P	·608	
FeO	36·23	36·20	36·21	Fe	·503	
MnO	8·95	8·96	8·96	Mn	·126	=·652
CaO	·08	·12	·10	Ca	·002	
MgO	·84	·83	·83	Mg	·021	
Li_2O	8·14	8·15	8·15	Li	·544	=·552
Na_2O	·31	·22	·26	Na	·008	
H_2O	·92	·82	·87			
Gangue	·82	·84	·83	Li_2	·272	=·276
	99·45	99·33	99·39	Na_2	·004	

These three localities, Bodenmais, Norwich and Grafton are, with that in Finland, the only ones at which the mineral triphylite has been found. In connection with these it is interesting to note the mineral lithiophilite from Branchville, Connecticut, recently described by Messrs. Brush and Dana* and shown by them to be analogous to triphylite in composition. This mineral was first analyzed by Mr. H. L. Wells of the Sheffield

* This Journal, III, xvi, 119, August, 1878.

Laboratory. I have made an analysis of it, but of another and slightly different variety, obtained by Messrs. Brush and Dana since the publication of their paper and from an independent, though closely contiguous deposit. The material was particularly fresh and free from alteration.

The results of the analysis are as follows:

(3.) Lithiophilite from Branchville, Conn.; color light clove brown; specific gravity =3·482.

	I.	II.	Mean.		Atomic relation.
P_2O_5	45·22	45·22	45·22	P	·636
FeO	13·10	12·92	13·01	Fe	·180 } =·631
MnO	31·93	32·12	32·02	Mn	·451 }
Li_2O	9·26	----	9·26	Li	·618 } =·628
Na_2O	·28	·30	·29	Na	·010 }
H_2O	·17	----	·17		
Gangue	·31	·28	·29	Li_2	·309 } =·314
				Na_2	·005 }
			100·26		

In the following table the atomic ratio for

$$P : \overset{II}{R}(=Fe, Mn, Ca, Mg) : \overset{I}{R}(=Li, Na)$$

is given for each of the three above analyses, and also for the Grafton mineral as analyzed by me (l. c.) and for the lithiophilite analyzed by Mr. H. L. Wells (l. c.).

	P	:	$\overset{II}{R}$	:	$\overset{I}{R}$				
Triphylite, Bodenmais,	·608	:	·652	:	·552	= 1	:	1·07	: 0·91
" Norwich, Mass.,	·630	:	·633	:	·632	= 1	:	1·00	: 1·00
" Grafton, N. H.,	·620	:	·651	:	·598	= 1	:	1·05	: 0·97
Lithiophilite, Branchville, Penfield,	·636	:	·631	:	·628	= 1	:	0·99	: 0·98
" " Wells,	·628	:	·632	:	·580	= 1	:	1·00	: 0·93

All of these approach very closely to ratio 1 : 1 : 1, required by the general formula

$$\overset{II}{R}_3P_2O_8 + \overset{I}{R}_3PO_4 \quad \text{or} \quad \overset{I\,II}{RR}PO_4.$$

The Bavarian mineral deviates most widely from this, and a similar though less pronounced deviation is seen in that found at Grafton. In all the five cases, however, it will be observed that the required ratio of 1:1·5 for $P : \overset{II}{R}+\overset{I}{R}_2$ is very nearly given, as is seen in the following table:

	P	$\overset{II}{R}+\overset{I}{R}_2$
Triphylite, Bodenmais,	1	: 1·52
" Norwich,	1	: 1·50
" Grafton,	1	: 1·53
Lithiophilite, Branchville, Penfield,	1	: 1·48
" " Wells,	1	: 1·47

In order to bring out the relations of the different varieties of triphylite to each other in respect to the amounts of iron and manganese contained in them, and also of these in turn to the allied species lithiophilite, the five analyses from which the above ratios were obtained are given together in the following table:

	Triphylite.			Lithiophilite.	
	(1.) Bodenmais.	(2.) Norwich.	(3.) Grafton.	(4.) Branchville (Penfield).	(5.) Branchville (Wells).
P_2O_4	43·18	44·76	44·03	45·22	44·67
FeO	36·21	26·40	26·23	13·01	4·02
MnO	8·96	17·84	18·21	32·02	40·86
CaO	·10	·24	·94	----	----
MgO	·83	·47	·59	----	----
Li_2O	8·15	9·36	8·79	9·26	8·63
K_2O	----	----	·32	----	----
Na_2O	·26	·35	·12	·29	·14
H_2O	·87	·42	1·47	·17	·82
Gangue	·83	----	----	·29	·64
	99·39	99·84	100·70	100·20	99·78

This series of analyses shows in a very striking way the transitions from triphylite, the phosphate of *iron* and lithium, to lithiophilite, the phosphate of *manganese* and lithium; to the first belongs the formula $LiFePO_4$ and to the second $LiMnPO_4$.

In closing this paper I wish to acknowledge my indebtedness to Professor George J. Brush, who has most liberally furnished me with the material needed for my examination.

Sheffield Laboratory, Dec. 16, 1878.

Art. XXVIII.—*Notes on the Mesozoic Strata of Virginia;* by Wm. M. Fontaine.

[Concluded from page 157.]

Petersburg Belt.—This exhibits many of the peculiar features seen in the Fredericksburg Belt, but on a more exaggerated scale. We do not however recognize more than one series. The strata are well exposed at Richmond in the lowest points reached by the erosion of the streams. They occur here with the Eocene and Miocene in place above them. They form the lowest visible beds in the river bank all the way down to Farrar's Island, and a short distance eastward of it. They also occur in a similar position in the banks of the Appomatox up to Petersburg, being in the vicinity of the rivers covered by only the lowest of the Eocene strata, the rest of the Tertiary having been removed. The canal cut by General Butler in the late war, through the narrow neck of Farrar's Island at Dutch Gap, in order to avoid the bend of the river, gives a good exposure. The description I shall give of the strata applies more particularly to the beds exposed at this last named locality, though it is applicable with some modifications throughout the belt. The beds incline gently eastward, and disappear under the Tertiary. It is not known how far they pass in this direction, but from their great comparative development where they disappear, they no doubt penetrate

far under the Tertiary. The lithology and stratigraphy present many remarkable features, only a few of which can be noticed. The thickness of the strata and coarseness of the component matters, strange to say, is greatest at the farthest point to the east at which they show themselves, viz: near Dutch Gap, although the source of the material is to the west. Not more than fifty or sixty feet of strata appear in the banks of the stream here, but from the manner in which the bluffs descend beneath the water, and the depth of the latter, the total thickness near Dutch Gap cannot be less than one hundred and fifty feet, while near Richmond it is not more than fifty or sixty feet. The variableness of the material, and the complexity of the rapidly shifting planes of false-bedding are so great, that no two sections taken a few feet apart will be similar; indeed the entire mass has the character of a "Kame."

The greater portion and the finer matter of the beds is a sort of incoherent grit, composed of grains of quartz, particles of feldspar in all stages of decomposition, and scales of mica, all mingled together without sorting and devoid of lamination, forming a mass without bedding, but penetrated by numerous and intricate false-bedding planes. This material which is plainly derived from the granitic and gneissic rocks to the west, is filled with small stones which are scattered confusedly throughout its mass. These are usually composed of quartz. While this is the more common material, we often find that locally, most of the material is the previously mentioned white, non-plastic earth, mingled with great quantities of small Potsdam stones, the whole being formed of the debris of Potsdam strata. Throughout the series, beds and pockets of large stones occur of very variable extent and thickness. These are much more abundant towards the top. They seem to be formed by the local removal of the normal grit, and the filling of the excavations with the stones. Thus we find that a mass of large stones, composing nearly all of the visible bank of the stream, will pass abruptly into the ordinary pebbly grit, or even fine sandy matter. Such pockets of stones are composed almost entirely of Potsdam quartzite and sandstones, well rounded and smoothed. The stones are more commonly under eight to ten inches, but they occur a foot or more in diameter. They are usually packed in a finer material composed of Potsdam debris. While these masses of stones are especially abundant in the highest portions of the series, they are not confined to this. Thus the lowest stratum that I saw exposed, was composed in one locality of rounded Potsdam stones, six, eight, ten and even twelve inches in diameter, packed as closely as possible together, and looking much like a section of the cobble-paved streets of Richmond.

Toward the top of the series, we find very commonly short layers, and nests of clay enveloped in the grit, but so sharply distinct from it that the clay seems to have been laid down on depressions in the surface of the grit during pauses in the deposition of this material. The distinctness is so great that at the line of junction of the two, the grains of the grit may be picked out separately from the clay. This clay sometimes, however, is interstratified for some distance with the sandy matter, the layers of sandy grit and clay alternating but not passing into each other. The clay is most commonly bluish or grayish in color, less commonly, pale reddish or liver-colored. It is very fine grained, highly plastic and tenacious, and thus contrasts strongly with the matter in which it is imbedded. The layers are rarely over a foot thick, and sometimes occur at short intervals apart, separated by the pebbly grit. They then often show the curious features of inclining at various angles toward each other, and of being cut off abruptly by the coarse matter. A seam of clay will thus sometimes abut against a nest of stones. In one place I saw three such short layers diverging from one extremity (where, however, they were not united) like the fingers of a hand, and ending abruptly in the normal pebbly grit. They do not usually extend horizontally more than twenty or thirty feet.

With these clay seams we often find rolled masses of similar matter, which sometimes have the diameter of four or five feet. These balls are enveloped in the grit, and commonly are attended with large Potsdam stones. The balls are plainly produced by the tearing up of the clay immediately after deposition. Sometimes a portion of a clay bed will be cut away by the eroding force, and rolled some distance and then covered by the floods of sandy matter carried forward by the agent which produced the erosion. These clays must have been brought from a very different source from that which yielded the coarse matter, and they seem to have been laid down on the irregular and shifting surface of the grit in pauses of the action of the force which produced this latter, hence the irregular inclination and local development of the clays. A renewal of the action of the powerful force which was involved in the transport of the coarser matters, would cut away and bury the clays. I do not find in place, any deposit which by its erosion could have yielded these clays. Their composition and character is just that of the thoroughly decomposed and slowly accumulated matter from granite and gneiss. I am strongly inclined to the opinion that they represent marshes within the Azoic area, which were swept away in the general and extensive erosion of its surface which took place at this time. This clay is of special importance as it contains the

plants to be mentioned farther on. Besides well-preserved plants, it contains much lignite and comminuted vegetable matter which gives it often a dark color. The plants are all drifted apparently from some distance, and but a small number are well enough preserved to be determined. The texture of the clay is so close that sometimes the entire substance of the plant is preserved.

The summit of the grit is sometimes partly laminated and contains occasionally a layer of clay, as if a pause in the sedimentation had taken place for a short time. On this, or on the summit of the grit, there occurs very generally, but not always, a very remarkable bed of stones packed in grit like that of the mass on which it rests. This latter is more or less eroded and trenched. This bed of stones is, I think, the representative of the surface stones mentioned above as occurring in the Fredericksburg Belt. It represents a force which swept over the Azoic on the west, as well as over the Mesozoic here, and in the Richmond Belt. It seems to have been the closing deposit of the Mesozoic series in these border belts for it is followed by the Eocene strata.

These stones form a most heterogeneous mass, a true drift deposit. The mass is of varying thickness, usually six to ten feet. Sometimes, however, it thickens up to fifteen or more feet and thins down to three or four, or disappears. The material is a mixture of gravel, large rounded stones, and more or less angular and large masses. The rounded stones are mostly Potsdam, usually four to six inches in diameter or under, but quite often one and two feet. The smaller stones are well rounded, the larger are sub-angular but still quite smooth. They often show Scolithus markings. Mingled with these, usually occurring at the top of the bed, we find the large blocks which are of Azoic rocks, and include varieties found at various points to the west as far as the Blue Ridge. It is noteworthy that this is the only horizon in the Mesozoic here, which shows any coarse matter but quartz, which would be furnished by the Azoic. The most numerous of the masses and the largest, are furnished by the granites and gneisses west of Richmond. Blocks of these, three to four feet in diameter are common, but sometimes they are ten and twelve feet. These are usually more or less abraded. Gneiss and mica schist from farther west occur in large slabs, sometimes showing but little wear. Some vein quartz from the Azoic, and fragments of the sandstones of the Richmond Coal field are seen, but not commonly. Masses of the peculiar chloritic and other schists of the Blue Ridge far to the west, are not rare. These often attain the dimensions of three or four feet, and are commonly but little abraded. None of these stones or those of the Fredericksburg

Belt, show glacial striæ, but some of them show a mode of wear and polish highly suggestive of ice action. Indeed it is not probable that these fragments would show striations even if they had been imbedded in ice moving over the land, for the surface was no doubt too deeply decayed, and in too incoherent a condition to produce scratches.

Besides the Potsdam, we find other material which has been brought from west of the Blue Ridge. Slabs of the characteristic fossiliferous layers of the Chemung occur, but rarely however. They contain impressions of the stems of Chemung Crinoids.

Fossil Plants.—Though the clays are full of vegetable matter, it is only locally, and in very restricted layers, that we find determinable plants. These are very interesting and sometimes beautifully preserved. They consist of Conifers, Ferns and Cycads. The Conifers sometimes retain their cones. They belong to several species, among which we may mention a plant very close to, if not identical with, *Widdringtonites Haidingeri* Etting., a Wealden species. Another is very near to or identical with, *Araucarites curvifolius* Etting., also a Wealden plant. The ferns and a Jeanpaulia, have a decided Wealden facies. But the most abundant and characteristic plant of the beds is a fossil identical with that fine Cycad, *Pterophyllum* (*Dionites* of Schimper) *Buchianum* Etting., which is a species of the Wealden of Germany. This exists in immense numbers, and shows compound leaves two feet long. I observed, imbedded in the grit composing the bank of the canal at Dutch Gap, a coniferous tree which in the part projecting from the earth is eighteen inches in diameter. Forty feet are said to have been cut off from it in opening and widening the canal. It is merely browned and does not yet seem to have reached the stage of true lignite. The wood seems to be like that of *Pinus strobus*, and shows the annual rings distinctly. It may be worked with tools, not having wholly lost its coherence. The grit contains only lignitic and silicified wood. Taken as a whole, the plants are decidedly Wealden. I did not see a single species similar to those of the Richmond coal field, though several seem identical with Fredericksburg species.

Surface deposits of the Azoic.—If we ascend from the river to the top of the hills near Dutch Gap, we find ourselves on an undulating plain, composed of Tertiary sands and clays, and possibly later beds. We observe no drift matter on this surface. If however we proceed westward, we find the Tertiary accompanying us until we approach the Azoic border, and still without surface drift. But in the vicinity of the Azoic, where the Mesozoic crops out from under the Tertiary, we find the Potsdam stones, Azoic erratics, fragments of Mesozoic sandstones,

etc., quite abundant, lying scattered over the surface. Passing still farther west, we find the same material with the fragments of the Richmond coal rocks becoming more abundant, and when we pass west of this field these coal rocks disappear, but we still have the other material. The Potsdam stones however do not pass westward over the whole Azoic area. They become confined to a belt along the James, in the upper course of that river. I do not know what is their disposition on the Upper Appomatox, as I have not examined the country in that quarter. It is plain that this coarse drift is the product of the same force which formed the uppermost bed of the Mesozoic at Dutch Gap.

The belt of Azoic, with the included Richmond belt of the Mesozoic, which contains on its surface Potsdam stones mingled with the fragments of Azoic rocks, is about twenty-five miles wide south of the James. It narrows to ten or twelve miles near the northern end of the Petersburg Belt, on the North Anna. West of this the Azoic drift masses are still found on the surface, up to the Catoctin Mountains but no Potsdam stones. It is noteworthy that this drift is usually almost entirely quartz, often in blocks two and three feet thick, representing the quartz of the veins so common in some of the Azoic strata. The reason why we do not find more of the crystalline schists is, because the deep decay which had affected these rocks at the time of their erosion, presented to the eroding agent only friable grits and clays with the quartz veins in them standing in their original position.

In the more westerly localities where the Potsdam is no longer found generally over the surface, I noticed that the bands of these Potsdam stones along the James do not follow the present windings of the stream but pass directly across the bends. They occur 150 to 200 feet above the river, and lie on the general level of the adjoining country.

The drift matter in the vicinity of the border belts, within the limits containing the Potsdam stones, covers the surface of the Azoic with a pretty continuous layer. But portions of it seem to be thicker than the general mass, corresponding to belts which were more deeply scored by the eroding agent. These have a direction a little north of west and south of east, showing the direction from which the eroding agent came. Sometimes, however, modern surface or stream erosion has so arranged these drift matters, as to cause them to appear to come from some other quarter than the west, which is their true source. This has misled Mr. R. P. Stevens, who visited the Richmond Coal-field some years ago, and came to the conclusion that the mode of transport was the movement of a glacier from the northward, in the Glacial Period. He states his views in a letter published in this Journal, Nov., 1873.

Professor Wm. B. Rogers, in an article entitled the "Gravel and Cobblestone Deposits of Virginia, and the Middle States," published in the Proc. Boston Soc. Nat. Hist., 1875, gives some account of the drift matter seen at Richmond, Alexandria, Washington, etc. Not having traced this material away from the vicinity of the James and Potomac, and finding it resting on the Tertiary, he was disposed to attribute its transport to the work of these streams in the Glacial Period, aided by the action of ice. While I think that most of this matter is of older date, yet I am convinced that considerable additions to that lying at the lower levels were made in the Glacial Period. I find drift matter on the Potomac, the James, and the Roanoke, which I think was brought during the Glacial Period, but this does not rise more than about sixty feet above the present water level. There is a great deal of drift along all these streams at that height, especially on the Roanoke.

The last of the surface deposits on the Azoic which are of importance are certain clays which are the most widely diffused of all. Thus we find them over the surface of the Azoic everywhere, to the west of the Petersburg Belt as far as the Catoctin and the Blue Ridge. They are not continuous but rather form irregular sheets. I cannot assert that these clays occur to the west of the northern end of the Fredericksburg belt, or that the Azoic drift in that quarter extends far westward of the eastern edge of the Azoic, as I have not made any extended examination there, but so far as I have examined, the condition of the surface is much as it is west of the Petersburg Belt. These clays are the only transported matter that I observed in the southern part of the State, in Pittsylvania County. Here they occur in the same way as in the northern district, now being described.

These clays are nearly always blood-red in color and rich in lime. They form a highly fertile subsoil, no matter on what sort of rock they may rest. They lie on all kinds of rocks, and are entirely independent of them. We find them on the gray grits of the Mesozoic; on the granite, with its surface decomposed to a white kaolinic grit, which shows the utmost possible contrast with the clays; or on mica-schists, etc. There is no rock east of the Blue Ridge rich enough in lime to yield these clays, except the epidotic schists, and hornblendic rocks which abound so between the Blue Ridge and the Catoctin. The clays contain decomposed fragments which resemble these strata, and when we can follow them, as we do up to these schists, we may conclude without hesitation that such is their origin. Hence they must have been transported in many cases, fifty or sixty miles. This however is not strange when we see that the Potsdam and other rocks from west of the Blue Ridge, must have

often traveled 100 miles and more measured in an air-line, and much farther, if the course of the rivers crossing the Blue Ridge be followed. Professor Rogers in the old survey of the State, noticed these clays and expressed the belief that they must have been transported from the west.

They lie at the same horizon occupying the highest levels, and mark the plane to which the country was cut down at the close of the Jurassic. We accordingly find them in the hill-tops and over the intervals between the rivers. But owing to this high level, they are often removed by erosion. These clays are usually accompanied by the coarse drift. When this is present they either lie on the drift or contain it within their deposits. The latter position of the drift stones however is rare. The clays pass up to the edge of the Tertiary but not over on it. They are conspicuous on the granite near Richmond, which when the clay covering is removed, presents rounded and smoothed surfaces. These red clays generally are covered by a greater or less depth of gray soil, usually not over a few feet.

General remarks and Conclusions.—It is a noteworthy fact that the belts of Mesozoic which yield coal, contain it mainly at their southern or higher extremities. This is true of the Richmond Belt, of the Prince Edward Belt, and of the Pittsylvania Belt, which terminates in the Dan River coal field in North Carolina. This was perhaps caused by the fact that the southern ends of the Mesozoic areas, being higher, were better fitted for the formation of marshes.

I think that many of the features described in the preceding pages can best be explained by supposing that, in Triassic and Jurassic times, the Appalachian Mountain region was receiving supplies of snow too great to be removed by melting. Consequently the excess must have been discharged by glaciers. These must have advanced and receded more than once in the earlier periods, but did not penetrate to the sea. Toward the close of the Jurassic they advanced in such force that they reached the sea. In the intervening time, while the ice was gathering force, ice rafts, charged with stones and earth, floated down the streams which issued from the foot of the ice. To the frequent pushing forward, and consequent abrasion of the matter accumulated at the foot of the ice, and in the upper course of the rivers, we must attribute much of the rounded and polished condition of the Potsdam stones now found so far to the east of their original position. This ice may have made its final advance over the whole of the portion of the Atlantic slope in which the features above described are found, or it may have issued from the Blue Ridge, mainly along the line of the Potomac and James, and then in its farther advance to

the east have spread laterally, so as near the border of the Azoic, to have coalesced into one sheet. The facts observed rather favor the latter method of advance. From this supposition it would follow that the Mesozoic areas were fed by the cold waters issuing from the ice and snow on the mountains. This may account for the paucity of animal life, especially Molluscan life, that they show. The only marine waters with which they could communicate contained forms that could not live in the cold inland waters.

It is probable that the courses of the present principal streams were marked out by this ice action, and hence come their direct course and independence of the character of the rocks over which they flow. There is no difficulty in explaining the growth of the plants, now found fossil, at a time when the Appalachian Mountain belt was covered with snow. All that was needed was a raising of the present winter temperature in the lowlands, for we shall show that for the formation of glaciers on the heights the climate need not have been colder than at present. Owing to the non-existence of the Rocky Mountains, the cold western and northwestern winter winds of the present time would not by reflection from that chain then reach the eastern slopes. At the same time the greater extension of the Gulf waters northward would cause southerly winds to sweep over these slopes. These winds passing over the cold waters of the lakes and great rivers would form abundant fogs. Thus a mild, equable, and moist climate would be produced in the lowlands, even if the earth had its present amount of cold, causing the growth of ferns, cycads, etc., and covering the hills with the immense growth of coniferous trees which we know must have existed. This condition of things would also have been eminently favorable for the production of coal. This was only brought to a close in the final advance of the ice at the end of the Jurassic Period, when all the abundant forms of plants of that period were extinguished to appear no more. No other cause seems adequate to explain the total extinction of the Jurassic flora, and the complete change which we find in the succeeding Cretaceous flora.

But while the plants were growing in the lowland and around the lakes, a very different condition of things prevailed in the high Appalachians. The stratigraphy of the formations composing this belt, and the amount of erosion which, as we know, took place, make it clear that in the early Mesozoic times much of this region must have stood above the snow line, and a still larger portion near it. If we recall the physical features of the North American continent which existed at that time, we shall see that even with our present climate then prevailing, the conditions would have been eminently favorable for the

formation of glaciers. Along with a sufficient degree of cold we must have abundance of moisture to produce glaciers. This would be supplied by the western and southwestern winds. The latter would sweep unchecked from the Pacific over vast bodies of warm waters in the interior, and meeting the lofty mountain belt of the Appalachians, would give unlimited supplies of snow. The configuration of this elevated district, with its broad slopes, and long valleys inclining in one direction, would be eminently favorable for the collection of snow and its discharge in the form of glaciers. Indeed this region must at that time have formed a perpetual storm center. We are not without evidence, however, that a period of cold greater than that now existing, prevailed toward the close of the Jurassic, and in this we find the explanation of the advance of the ice so far to the east at that time.

Mr. Judd, in his excellent paper on the Secondary Rocks of Scotland, published in the Quar. Jour. Geol. Soc., vol. xxix, gives a description of certain very remarkable beds of stones, which under the name of the "Brecciated Beds of Ord," form the uppermost beds of the Upper Oolite of the east coast of Scotland. These beds are composed of huge masses of stones brought from a distance, and packed in fine argillaceous matter. The conditions are such that it is impossible for water alone to have transported the material. Hence some of the English geologists attribute this formation to ice action. The beds of stone are interstratified with shale beds, containing a beautiful Oolitic flora. Many of the features seen here resemble those of the beds at Dutch Gap. The question suggests itself whether at the close of the Jurassic an ice sheet did not cover the northern part of Scotland, extending east and west. If so, we may have in the melting of this ice, the source of supply of those waters which formed the fresh water Wealden deposits of England and northwestern Germany. It is well known that it is a puzzle to determine whence the great rivers which fed these lakes could have derived their abundant waters.

Another question may be asked. Is it not possible that some of the drift of our northern States, which is attributed entirely to the action of forces in the Glacial Period, may be much older? It is well known that none of the Mesozoic areas of the Atlantic slope seem to have had perfect communication with the sea. May not this be due, at least in the latest formed of them, to the great quantity of drift matter accumulated seaward, and not in all cases to an elevation of the coast?

The question whether drift deposits, similar to those of Virginia, may or may not be found in the States lying around the southern prolongation of the Appalachian belt, is worthy of the attention of geologists. Sir Charles Lyell, in his account of his second journey to the United States, mentions some

significant features, which in Georgia and Alabama, attracted his attention on the line of meeting of the Tertiary and Azoic. He mentions seeing erratic masses of various sizes and character, which rest on the Azoic, pass up to the Tertiary, but do not pass over on the latter. He noticed this drift as resembling the northern Glacial Drift. If this is of Upper Jurassic age, then it ought not to pass over the Tertiary but under it. Again, he mentions seeing in Alabama, at the base of the Cretaceous, "dense masses of shingle perfectly loose and unconsolidated, derived from the waste of Paleozoic strata, a mass in no way except by its position, distinguishable from ordinary alluvium."

Tuomey mentions at Aiken, in South Carolina, beds of gravel and sand, without fossils, and lying at or under the base of the Cretaceous. Lieutenant Vogdes thinks that these may be Wealden.

Morgantown, West Virginia, Nov. 4, 1878.

ART. XXIX.—*Notice of recent Additions to the Marine Fauna of the eastern coast of North America, No. 3*; by A. E. VERRILL. *Brief Contributions to Zoology from the Museum of Yale College.* No. XL.*

AMONG the many interesting additions recently made to the American marine fauna, are two handsome undescribed species of "sea-feathers" (Pennatulaceæ) and three fine new species of hydroids, belonging to the family Plumularidæ, of which only three species have previously been known to inhabit our northern coast. Those now added appear to belong to the genus *Cladocarpus*, recently established by Allman, for several deep-water forms, from the coasts of Europe and the Gulf of Mexico. Two very interesting Cephalopods have also been recently added to our fauna.

ANTHOZOA.

Virgularia grandiflora, sp. nov.

A large, stout species, with very large polyps, which are only slightly united, close to their bases. Rachis stout, ventral side convex, and with a wide naked space; below the polyp-iferous portion there is a marked fusiform swelling, with longitudinal wrinkles; the end is bulbous and perforated; at the distal end the naked rachis extends about 8^{mm} beyond the last polyp-cells, and tapers to a blunt tip. Axis rather stout, rounded, yellowish white. The polyp-cells are large, and arranged in very oblique, rather irregular rows, seldom containing more than six or eight, and separate so nearly to the

* Nos. XXXVII and XXXVIII were, by an error, doubly employed in this series. This is, in reality, the forty-second article.

base that they form only very rudimentary alæ, while in some states of contraction they appear entirely disunited. The rows are rather distant on the same side, and on opposite sides alternate irregularly, and the dorsal members of adjacent rows intermingle or overlap, while the polyps in the middle region usually stand alternately farther forward and farther back in the same row, while the most ventral one is usually placed farther toward the ventral side in each alternate row; the polyp-cells are stout, cylindrical, soft, slightly eight-ribbed, considerably contractile. Tentacles, very long, tapering to a long slender tip, with very numerous, slender lateral lobes. Most of the polyps are well expanded; a very few have the tentacles nearly retracted. Zoöids are numerous, small, but very distinct, covering the lateral intervals between the rows of polyps and usually extending into the intervals between their bases, while along the ventral side the lateral patches are usually connected by a belt of zoöids running outside of the most ventral polyp-cells, but those of the latter that stand out more than usual, usually interrupt these belts. No calcareous spicula were found in the polyps or cœnenchyma. Color of the rachis and polyp-cells brownish yellow, in alcohol; tentacles dark brownish red.

Length, 350^{mm}; of naked peduncle, 60^{mm}; diameter of bulb of latter, 10^{mm}; of narrow portion, 5^{mm}; of rachis, 5 to 8^{mm}; of axis, 1·5^{mm}; diameter of largest polyp-cells, 3^{mm}; their length, 5^{mm}; length of tentacles, 6 to 7^{mm} or more.

Taken on a trawl line, in 220 to 260 fathoms, lat. 42° 46′; long. 63° 45′, by the crew of the schooner "Laura Nelson," Capt. R. N. Morrison. This is a very handsome and remarkable species, of which only a single specimen has been obtained. It differs widely from the previously described species of *Virgularia*, and approaches Kölliker's genus *Halipteris*, in appearance, but differs in the character of the polyp-cells and in the absence of spicula.

Funiculina armata, sp. nov.

A long, slender species, with large, rigid, urceolate, spiculose polyp-cells, armed at the aperture with eight, sharp, divergent spiculose points. Axis and rachis quadrangular, slender, the sides of the axis concave. The polyp-cells are entirely separate and arranged in numerous irregular transverse clusters of two to four, smaller and larger intermingled: they are so stiffened by spicula as to be scarcely flexible, and retain well their form: they are elongated, swelling out gradually from near the base and tapering again, above the middle, to near the summit, which suddenly expands to the edge, from whence eight, long, acute, rigid, white points diverge; tentacles spiculose, but wholly retractile within the cells. Among the ori-

dary cells are small clusters of much smaller zoöids, which are white, prominent, larger at summit than at base, and possess eight rudimentary tentacles. The polyp-cells are filled with long, slender, smooth, triquetral, or three-winged, prismatic spicula, arranged in longitudinal bands and irregular transverse clusters, while a convergent cluster forms each of the eight points at the summit; similar, but smaller, spicula occur in the main stem of the tentacles. The ova are large and situated both in the polyp-cells and in the cavity below their bases, where a considerable cluster can be seen when, as often happens, the cells are broken off. Rachis yellowish white below, light orange-brown above; polyp-cells darker brown, with white spicula; tentacles dark brownish red.

Height, 600^{mm}; diameter of rachis near base, 1·5^{mm}; in middle, about 1^{mm} to 1·5^{mm}; length of larger polyp-cells, about 6^{mm}; diameter, 1·5 to 1·75^{mm}.

Taken on a trawl-line in 300 to 400 fathoms, about forty miles southwest from the N.W. Light of Sable Island, N. S., by George K. Allen, schooner "M. H. Perkins."

Ptilella borealis Gray = *Pennatula borealis* Sars = *Pennatula grandis* Ehr. (*non* Pallas) = *Ptilella grandis* Kor. & Dan.

Several additional specimens of this species have been received from off Nova Scotia.

In the Fauna Litt. Norvegiæ, vol. iii, p. 82, 1877, Koren and Danielssen have given an elaborate description of this species, with figures. They have shown good reasons, apparently, for adopting Gray's genus, *Ptilella*, for this form, but inasmuch as Ehrenberg's name (*P. grandis*) had been preoccupied, it ought not to be used for this species.

Cephalopoda.

Histioteuthis Collinsii, sp. nov.

A very large and handsome species, with a broad thin web, extending between and nearly to the ends of the six upper arms. Tentacular arms about two feet long and slender, expanding near the end into a broad, long-oval, sucker-bearing portion "or club," which is bordered by a membrane, widest on the outer edge; it ends in a tapering tip, on the back of which there is a thin crest-like membrane or keel, enlarging backward to the end, where it forms a rounded lobe. The most expanded portion of the "club" bears five rows of suckers, with finely serrate rings; two rows contain much the largest suckers, four or five in each, the more central of the two rows containing four suckers larger than the rest; outside of these are two rows of medium-sized suckers, and along the inner edge of

the club there is one partial row of similar ones, while along the inner edge of the proximal portion of the club there is a row of smooth-edged suckers, alternating with tubercles that fit into corresponding suckers on the other arm; a row of similar but smaller suckers extends for about six inches along the inner median line of the arm, alternating, two by two, or singly, with tubercles, and gradually becoming more distant. The tip of the arm, beyond the expanded club, bears minute serrate suckers, at first in six rows, decreasing to two at the end. Sessile arms stout, three-cornered, tapering to slender tips, each bearing two rows of globular suckers, having a small, oblique opening and few blunt teeth.

All the arms on the left side are an inch or more longer than the corresponding right ones. The dorsal and ventral arms, of the same side, are about equal, and decidedly shorter than the two lateral pairs, which differ but little in length. A broad, thin web, about two-thirds as broad as the length of the arms, unites the upper three pairs together, and as a narrowing border extends along their sides, nearly to the tips. The lower lateral arms have a thin, crest-like membrane on their outer, median surface, commencing at the basal fourth and extending nearly to the tips. The ventral arms are united together, near the base, by a web, which also unites to the main web, in the median plane. A narrow web, arising from the outer angles of the arms, also unites all the arms together for a short distance above their bases. Eyes mutilated, their lids form a large, simple, rounded opening. Beak with very sharp black tips; a broad membrane, rising into six prominent angles, surrounds the mouth. The outer surface of the head and arms is covered with large, very slightly raised warts or tubercles, which are dark blue with a whitish center; a circle of them surrounds the eye lids. Color, between the warts, purplish brown, with dark brown spots, and reddish specks; web and inner surface of arms uniform dark reddish brown; suckers yellowish white; tentacular arms light orange-brown.

Tentacular arms 24 and 25 inches long; diameter at base, ·5; breadth of club, ·70, without membrane; its length, 2·75; length of the slender tip, 1·25; of dorsal crest, 1·5; length of dorsal arm, of left side, 14 inches; 1st lateral, 17; 2d lateral, 17·25; of dorsal, 14·25; breadth of lateral arms, at base, ·9; thickness, ·75; diameter of eye-opening, ·9; diameter of head, at base of arms, 3·5.

Taken from the stomach of *Alepidosaurus*, lat. 42° 49′, long. 62° 57′, off Nova Scotia, by the crew of the schooner "Marion," Capt. J. W. Collins.

All parts back of the eyes are absent, the eyes are mutilated, but the specimen is otherwise in excellent preservation, even

the thin web and the colors being uninjured. With it was the terminal portion of an arm of a gigantic squid (*Architeuthis*), too much injured for specific determination. I have named the species in honor of Capt. Collins, to whom and the crew of his vessel, we are indebted for so many interesting and novel specimens.

Taonius hyperboreus Steenst. (?)

A large and handsome species of this genus has just been received, which may be this species, though in its proportions differing from Steenstrup's measurements. The eyes are very large and globular, in contact, beneath the head. The arms are very short, and part of them have lost their tips and afterwards healed. The tail is long, lanceolate, tapering to a very long, slender, acute tip. Color brownish red, with rather large rounded, dark brown spots. Length of head and body, 13·5 inches; edge of mantle to tip of tail, 12; tail, 5; its breadth, 1·8; diameter of body, 2 25; of eye, 1; length of ventral arms, 1·9; of lower lateral arms, 2·25.

Taken at the surface, in the northern edge of the Gulf Stream, W. long. 55°, by Mr. Thomas Lee, schooner "Wm. H. Oaks," Jan., 1879.

ART. XXX.—*Note on the Age of the Laramie Group or Rocky Mountain Lignitic Formation;* by H. M. BANNISTER.

IN his recently issued report on Systematic Geology, vol. i, U. S. Geological Exploration of the 40th Parallel, Mr. Clarence King discusses at length the disputed question of the age of the Rocky Mountain lignite series, to which, by agreement with Dr. Hayden, he gives the name of the Laramie group. In his argument for the exclusively Cretaceous age of these beds, it appears to me that he has generalized too freely, and I propose to notice a few omissions and points where he seems to me to be in error.

We may leave aside all the paleontological evidence of the age of these beds, the general Eocene aspect of their invertebrate fauna, which, I believe, no one has disputed, the Tertiary *facies* of the flora, which, I believe, is an issue between Professors Lesquereux and Newberry, and the Cretaceous Saurians found high up in the series, as all compatible with the theory of its transition character, and the question of its conformability remains. Mr. King asserts that it is absolutely unconformable with the overlying Vermilion Creek group, and that nowhere, save in one locality, is there any appearance of conformability between the two. I should hesitate to disagree with him, were

it not that when examining this formation with the late Mr. F. B. Meek, in the year 1872, we both thought we found evidences of conformability between these strata and the overlying Tertiary in other localities than Black Buttes, the one admitted by Mr. King. Then, in examining the reports of other geologists who have visited the region, I find that their observations in these localities, as far as stated, agreed with ours. Among these I may perhaps include Mr. King himself. He says (p. 334), "Good exposures of the Laramie group beds may be seen along the railroad, just east of Separation station, where they show the peculiar ashen gray sandstones, containing a considerable development of argillaceous beds, and a great number of coal seams, and contain plentiful plant remains, generally as leaf-impressions, and frequently also as indistinct and partially carbonized stems in the impure sandstones. In the ridge south of this station, they dip at an angle of 10° north, but flatten out to the north, so *that the line between them and the overlying Tertiaries is even more difficult to determine* than the exact division between them and the underlying Fox Hills group." (Italics mine.) Now, at this locality, Mr. Meek and I examined a thickness (estimated) of eighteen hundred feet or more of conformable beds, and in the upper portion found abundance of purely fresh-water shells, and a fragment of bone, apparently of a turtle. At the base of this series was a heavily bedded sandstone, exactly similar to that which forms the only constant horizon at Black Buttes, and the overlying strata were not dissimilar to those at the latter locality.

In the report on Descriptive Geology of the same survey, Mr. Emmons, Mr. King's assistant, when speaking of this same region, uses the following words (p. 207, vol. ii, U. S. Geol. Expl. 40th Parallel): "To the west of Rawling's Peak, as we have seen, the Cretaceous strata fall off with an ever decreasing angle of dip, assuming to the north of the railroad, between Separation and Washakie, an almost horizontal position, and are gradually succeeded by the overlying, and, in this region, conformable beds of the Vermilion Creek Tertiary." This passage sufficiently indicates that the conformability of the strata is not merely local; and that it is also not local at Black Buttes, is proven by the testimony of Professor Cope, who found it to exist for miles south of the station, while our observations were made to the north. Thus we have a conformability, as far as observed, on both sides of the Washakie Tertiary basin, according to the testimony of Mr. King's own survey.

Passing to the westward, we find Mr. King speaking as follows in regard to the Evanston coal-bearing strata which, by Mr. Meek and myself, had been regarded as of uncertain but probably of Tertiary age. "At Evanston the highest portions

of the Laramie Cretaceous are not exposed, but the sandstones near the summit of the group contain the enormous workable coal-beds of the Rocky Mountains and Wyoming Coal Companies. These coal-bearing Laramie beds dip at angles from 16° to 25°, whereas the Vermilion Creek Tertiaries are nearly horizontal over them, and carry remains of the genera *Coryphodon* and *Eohippus* and fishes." Mr. Emmons says, speaking of the same beds, that there is no doubt but that they belong to the Cretaceous and probably to the Laramie group, though, from the lack of paleontological evidence, this reference is simply conjectural. Now, so far as I can learn, not a single salt- or brackish-water fossil has been found at this locality, though land and fresh-water shells are abundant in strata immediately above the coal, the flora is said to be Tertiary, and in these same dipping beds—not in the nearly horizontal ones mentioned by Mr. King—Professor Cope found an Eocene vertebrate fauna, identifiable with that of the lower Green River epoch (Vermilion Creek Tertiary.) Moreover, if the coal is near the top of the Laramie series, which here, according to Mr. King, dips unconformably under the Tertiary, what are we to do with over twenty-four hundred feet of strata overlying it conformably and of which a section was taken by Mr. Meek and myself? As a detailed section it is of no special value, the strata vary sometimes within a very short distance, but its vertical extent is of interest in this connection. It is highly probable that it includes the horizon of the limestone bed with vertebrate remains discovered by Professor Cope, which was possibly only local; such beds sometimes run out in a few rods or yards. A comparison of the lower part of the section made at Almy, by Mr. Meek and myself (p. 540, Hayden's Annual Report for 1872) and one including the same horizons also made at Almy, which is given by Professor Lesquereux, on page 338 of the same volume, will sufficiently illustrate this fact. The sandstones, indeed, often suggested beach deposits rather than regularly deposited strata.

That these Laramie beds and the overlying Tertiaries are often unconformable may be readily admitted without invalidating the hypothesis of their transition character. They are sometimes unconformable in themselves; an apparent nonconformity was noted just below the horizon of Black Buttes, and another, many hundred feet lower down, has been made the dividing line between the Tertiary and Cretaceous by Major Powell and Dr. White. The evidence appears to me to indicate that the Laramie epoch was throughout one of stratigraphic disturbance rather than that there was only one great orographic change at its close, and that the reasonableness of the opinion of its transition character is not yet altogether disproved.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Gelatinous Silica and on an Inorganic membrane formed of it.*—Ullik has experimented on gelatinous silica and has prepared it in such form as to be used in dialysis. The transparent jelly which is obtained when a dilute solution of water glass is poured into hydrochloric acid, is well known. The author has succeeded in washing it absolutely free from sodium chloride and the excess of acid, and in producing pure silicic acid as a perfectly transparent jelly. In the air it dries to a transparent mass, about one-fifth in volume, breaking into fragments resembling hyalite, and pretty hard. Four decigrams of the dry mass gave when evaporated with hydrogen fluoride, a residue of only 0·0016 gram. It corresponds nearly to the formula H_2SiO_3. Suddenly ignited, it gives a brilliant sand; but heated with care, the temperature rising very gradually, up to full ignition, it retains its form and glassy transparent masses are obtained. The dried has a sp. gr. of 1·84 to 1·907, resembling opal; the ignited, of 2·322 to 2·324, which is that of tridymite. The facility with which the gelatinous silica was washed led the author to conclude that diffusion had taken place through it and hence that it possessed the properties of a membrane. To prove this definitely, the bottom was cracked out of a precipitating glass, the edges pressed down upon a piece of rubber covered with filter paper, and the mixture poured in. After solidification of the silicic acid, the whole was immersed in water, the filter paper removed, and the gelatinous mass washed by repeated renewal of the water. In this way a dialyser was made whose mouth was closed completely by a membrane of silicic acid three millimeters thick. A second one had a membrane half a millimeter thick, supported by paper. Experiment proved that no filtration took place through these membranes, but that they allowed diffusion to go on readily. For example, 50 cm. of a 20 per cent sugar solution being placed in the dialyser, it being immersed in water until the level was the same within and without, there was produced in three days a difference of level of 12 mm. with the thicker, and 20 mm. with the thinner membrane; and there had passed out into the water 14 per cent of the sugar in the former case and 49·5 per cent in the latter. A membrane made in a funnel tube, and this filled with copper sulphate solution and placed in water, showed after twenty-four hours a difference of level of 13 cm. and the water without was distinctly blue.—*Ber. Berl. Chem. Ges.*, xi, 2124, Dec. 1878. G. F. B.

2. *On the Action of Hypo-chlorous oxide on Ethylene.*—Mulder and Bremer have studied the action of hypochlorous oxide upon ethylene. A flask filled with fragments of pyrolusite was heated on the water-bath, and strong hydrochloric acid dropped upon it through a syphon. The chlorine thus evolved was con-

ducted through three wide glass tubes each of one-half a liter capacity, containing equal weights of pounded glass and of mercuric oxide, and heated in a sandbath to about 340°. The hypochlorous oxide thus produced was mixed with dry ethylene gas in excess and passed upward through a vertical tube cooled by water and protected from the light. A liquid condensed in the tube which afforded on fractioning a small quantity of ethylene chloride boiling below 100°, but consisted chiefly of a product boiling at 180°–210°, and having a vapor density of 73·8. Analysis gave its empirical composition as $C_4H_6Cl_2O_2$. Fractionated in vacuo, a purer product was obtained. Heated with silver oxide, the filtrate afforded a salt which proved to be silver monochloracetate $CH_2Cl—COOAg$. Heated with water (in which it is not soluble) with an upward condenser, monochloracetic acid was again obtained. After neutralizing the liquid with potassium carbonate, ether extracted from the liquid monochlorhydrin $CH_2Cl—CH_2OH$, boiling at 130°. Hence the action described affords a body having the structural formula $\begin{matrix} CO—O—CH_2 \\ | \qquad\quad | \\ CH_2Cl \quad CH_2Cl \end{matrix}$. The reaction by which this monochlorethyl monochloracetate is produced is

$$\left.\begin{matrix} CH_2 \\ \| \\ CH_2 \end{matrix}\right\rangle + \left.\begin{matrix} Cl \\ Cl \end{matrix}\right\rangle O = \begin{matrix} CH_2—O—CH_2 \\ | \qquad\quad | \\ CH_2Cl \quad CH_2Cl \end{matrix}$$

as the first stage; and

$$\left.\begin{matrix} CH_2—O—CH_2 \\ | \qquad\quad | \\ CH_2Cl \quad CH_2Cl \end{matrix}\right\rangle + \left.\begin{matrix} Cl \\ Cl \end{matrix}\right\rangle O = \begin{matrix} CO—O—CH_2 \\ | \qquad\quad | \\ CH_2Cl \quad CH_2Cl \end{matrix} + (HCl)_2$$

as the second. No products resulting from the action of one molecule of hypochlorous oxide upon one of ethylene appear to be formed.—*Ber. Berl. Chem. Ges.*, xi, 1958, Nov. 1878. G. F. B.

3. *On the Hydrogenation of Benzene.*—The tendency in some quarters to consider benzene as a final hydrocarbon, in consequence of its being the nucleus of the aromatic series, has led BERTHELOT to investigate the action described several years ago by him, by which any unsaturated hydrocarbon may be converted into a saturated one. The method consists simply in treating the hydrocarbon with a saturated solution of hydriodic acid at a temperature of 275° to 280° for some time. In the new experiments, several tubes, each containing 20 c.c. of saturated hydriodic acid and 0·6 c.c. of benzene, were sealed and heated to 270° for twenty hours. The unaltered benzene was removed by fuming nitric acid, and the remaining hydrocarbon, about two thirds of the whole, rectified and analyzed. It was a mixture of C_6H_{10} and C_6H_{12}. A second treatment with hydriodic acid gave nearly pure C_6H_{12}. A third gave a mixture of C_6H_{12} and C_6H_{14}. And finally a fourth treatment gave C_6H_{14} boiling between 68·5° and 70°. This is the final term identical from both the aromatic and the fatty series, the only completely saturated hydrocarbon containing six atoms of hydrogen.—*Ann. Chim. Phys.*, V, xv, 150, Oct. 1878. G. F. B.

4. *On the Action of Hypobromous acid on Ethylene dibromide.*—DEMOLE has been led by theoretical views to study the action

of hypobromous acid on ethylene dibromide. This latter body treated with a strong solution of the acid in excess and agitated for an hour, gave an oily liquid and a supernatant liquid colored by bromine. The latter on examination afforded bromacetic acid. The oil heated in a watch glass for four or five hours, crystallized on cooling, the mass being purified by crystallization from boiling alcohol. It then gave the formula $C_4H_2Br_8O$, and this by its reactions was proved to be an acetone, having its oxygen joined to the carbon by two bonds. On reduction with sodium amalgam, ethyl-methyl-acetone C_4H_8O was obtained, which gave a crystalline compound with hydrosodium sulphite. Hence the first body was hexa-brom-ethyl-methyl-acetone. Oxidized with fuming nitric acid, it yielded malonic acid, easily determined from its properties and those of its barium salt. This shows the structure of the hexa-brom-ethyl-methyl-acetone to be $CBr_3—CO—CH_2—CBr_3$, the terminal carbon atoms having all the bromine.—*Bull. Soc. Ch.*, II, xxx, 482, Dec. 1878. G. F. B.

5. *Relative Affinities of Oxygen and the Haloid Elements.*—In the October number of the Annales de Chimie et de Physique, Berthelot has a very interesting article on the Relative Affinities and Power of Replacement of Oxygen and the Haloid Elements, and shows that the order of affinity and replacement can be predicted from the amount of heat evolved in the production of the various binary compounds, of which the metals and the metalloids form with oxygen on the one part, and with chlorine, bromine or iodine on the other. In the case of the metals the heat evolved by union with oxygen is with a few exceptions less than that resulting from the union of the same metal with either of the three haloid elements mentioned, and it is shown that in the same measure oxygen may be replaced by the direct action of these elementary substances on the oxides; and further that, in the exceptional cases presented by certain compounds of iodine, where the thermal relations are reversed, the order of affinities—as shown by the replacing power—is reversed as well. Moreover a few other apparent exceptions to the general rule are shown to result from the formation of intermediate products.

While, however, the heat evolved by the combination of the metals with oxygen is less than that resulting from the formation of the corresponding haloid salts, these conditions are, as a rule, reversed in the case of the compounds of the metalloids with the same elements, and so the order of replacement is reversed as well. This is shown to be true in regard to the compounds of phosphorus, arsenic, silicon and boron, and moreover it is made evident that certain anomalies, depending on the production of oxichlorides, are really confirmations of this law of thermo-chemistry, which Berthelot has done so much to establish. The paper is one of great interest, and contains besides the illustration of the important general principle we have stated, the experimental evidence of a large number of new thermo-chemical data. Especially worthy of notice is a new general method of thermo-chem-

istry applied to the determination of the heat of combination of bromide of aluminum, that of chloride of aluminum being known. Thus Al_2Cl_6 is dissolved in water containing 6KBr, and on the other hand Al_2Br_6 is dissolved in the same amount of water containing 6KCl. The difference between the quantities of heat evolved in the two cases gives the only element wanting for calculating exactly the difference between the amounts of heat evolved in the formation of the two anhydrous compounds. Thus

We know from previous investigations that $3(K_2+Br_2$ (gas)+water)$=6KBr+Aq.$ yield ..... +285·0 units.

We find by experiment at this time that Al_2Cl_6 dissolved in the previous solution yield ..... + 76·0 "

Representing the heat resulting from the union of $Al_2+Cl_6=Al_2Cl_6$ by } x

We have for $Al_2Cl_6+6KBr+Aq.$ ..... $x+361·0$ "

So also we know that $3(K_2+Cl_2+$water$)=6KCl+$ Aq. yield ..... +302·4 "

And we find as before that Al_2Br_6 dissolved in this product yields ..... + 86·9 "

Representing the heat-form $Al_2+Br_6=Al_2Br_6$ by ... y

We have for $Al_2Br_6+6KCl+Aq.$ ..... $y+389.3$ "

Since the final states of the two resulting solutions seem absolutely the same, it must be that

$$x+361·0=y+389·3$$

$$\text{or } x-y=389·3-361·0=28·3 \text{ units.}$$

If then we adopt for x (when $Al_2+Cl_6=Al_2Cl_6$) +160·9

We have for y (when $Al_2+Br_6=Al_2Br_6$) +132·6

J. P. C.

6. *The Part of Acids in Etherification.*—The effect of the presence, merely, of an inorganic acid in determining the etherification of alcohols by the organic acid has long been known, and the theories advanced to explain this so-called "catalytic" action have been numerous. In a paper following the last, Berthelot seeks to show that these processes also conform to the general "law of maximum works," that is, are the natural result of a tendency to a condition, in which the maximum amount of heat is evolved. To this end both the chemical, and the thermal conditions, involved in both the separate and also the mutual action of hydrochloric and acetic acids on alcohol are fully discussed, and it is shown that in the last case the formation of acetic ether is attended with a much greater evolution of heat than would be that of hydrochloric ether. Hence when the two acids are present, the former and not the latter of these two ethers is the chief product of the reaction, and it is shown that the one condition which chiefly determines the result is the great amount of heat evolved by the solution of HCl gas in an excess of alcohol. It is also shown that this thermal theory gives a satisfactory account of the well known conditions limiting these reactions, and that in the action of a mixture of strong nitric and sulphuric acids on hydrocarbon compounds, similar thermal relations determine the production of nitro- instead of sulpho-derivatives. J. P. C.

7. *Preliminary Note on the Substances which produce the Chromospheric Lines*;* by J. NORMAN LOCKYER, F.R.S.—Hitherto, when observations have been made of the lines visible in the sun's chromosphere, by means of the method introduced by Janssen and myself in 1868, the idea has been that we witness in solar storms the ejection of vapors of metallic elements with which we are familiar from the photosphere.

A preliminary discussion of the vast store of observations recorded by the Italian astronomers (chief among them Prof. Tacchini), Prof. Young, and myself, has shown me that this view is in all probability unsound. The lines observed are in almost all cases what I have elsewhere termed and described as *basic lines*; of these I only need for the present refer to the following:

b_3 ascribed by Ångström and Kirchhoff to iron and nickel.
b_4 " " Ångström to magnesium and iron.
5268 by Ångström to cobalt and iron.
5269 " " calcium and iron.
5235 " " cobalt and iron.
5017 " " nickel.
4215 " " calcium, but to strontium by myself.
5416 an unnamed line.

Hence, following out the reasoning employed in my previous paper, the bright lines in the solar chromosphere are chiefly lines due to the not yet isolated bases of the so-called elements, and the solar phenomena in their totality are in all probability due to dissociation at the photospheric level, and association at higher levels. In this way the vertical currents in the solar atmosphere, both ascending and descending, intense absorption in sun-spots, their association with the faculæ, and the apparently continuous spectrum of the corona and its structure, find an easy solution.

We are yet as far as ever from a demonstration of the cause of the variation in the temperature of the sun; but the excess of so-called calcium with minimum sun-spots, and excess of so-called hydrogen with maximum sun-spots follow naturally from the hypothesis, and afford indications that the temperature of the hottest region in the sun closely approximates to that of the reversing layer in stars of the type of Sirius and α Lyræ.

If it be conceded that the existence of these lines in the chromosphere indicates the existence of basic molecules in the sun, it follows that as these lines are also seen generally in the spectra of two different metals in the electric arc, we must be dealing with the bases in the arc also.

8. *Upon the Nature of Spectra of Mixed Gases.*—Prof. E. WIEDEMANN discusses at length the theories of the action of molecular forces in producing various spectra. Certain writers, among whom are Stefan and Van de Waals have supposed the existence of an attracting force between the molecules, and also a repulsive one arising from an ether envelope which surrounds the molecule. The rotation and oscillation of the atoms in the mole-

* Paper read at the Royal Society, London.

cule, which increase to the point of breaking up the molecule into its atoms and which action increases with the temperature, may produce periodic vibrations in the surrounding light ether. Line spectra, on this theory, are produced when, under the influence of heat, the molecules are divided into atoms and an oscillatory vibration is set up. F. Lippich and Pfaundler have shown from a theoretical point of view that the broadening of line spectra may result from the influence of high temperature in producing a motion of the molecules of a gas partly toward and partly away from the observer. Wiedemann examines the views in regard to band spectra and concludes that they cannot be attributed to rotation of the molecules; and that the difference between band and line spectra is not explained on the hypothesis of a change of pressure of the gas under consideration. Wiedemann believes, after consideration of the facts, that the difference between emission and absorption spectra is to be sought in the change of vibration peculiar to each chemical combination, which vibration is modified by surrounding circumstances; and that two different substances can give identical spectra only when the forces acting between the atoms in the molecules are identical. Prof. Wiedemann states his intention to test his theoretical conclusions by experiment and give the results of his preliminary investigation upon the spectra of mixed gases. A Geissler tube containing a small portion of mercury was filled with hydrogen and the tube was heated in an air bath while the current from an induction apparatus passed through it. At ordinary temperatures the hydrogen spectrum was seen; with an increase of temperature appeared that of quicksilver. With increasing temperature the spectrum of the latter grew brighter and brighter, while that of hydrogen disappeared in all parts of the tube. In another trial, sodium was enclosed in hydrogen or nitrogen vapor. Here, also, the spectra of the gases disappeared and at a high temperature only the lines of sodium appeared. The author believes that the disappearance was not due to any new chemical combination into which the substances had entered, and discusses at some length the theory that the passage of electricity from molecule to molecule is to a certain degree selective and that it can call forth certain oscillations or vibrations in certain molecules of one substance while it cannot do so in a neighboring substance which forms a mixture with the first.—*Ann. der Phys. und Chem.*, No. 12, 1878, p. 500. J. T.

9. *The Law of the Telephone.*—M. HERMANN, in Archiv für Physiologie, vol, xvi, pp. 264 and 314, has adduced certain experiments to show that M. Dubois Reymond's theory that the action of the telephone can be explained from the general law of induction in which the bending of the iron plate is taken into account and the induction of the current path upon itself is neglected, does not explain the facts observed. Prof. H. F. Weber communicated to the Züricher Naturforschenden Gesellschaft, a paper in which he showed that Hermann's experiments agreed entirely

with the theoretical laws of induction, and that M. du Bois Reymond was wrong in neglecting the induction of the current path upon itself, which last was really the principal agent in producing the agreement between theory and practice. Ten days later M. Helmholtz presented a paper to the Berlin Akademie der Wissenschaften which covered the same ground as Prof. Weber's paper. The general results of these papers are as follows: (1.) "In the telephonic circuit the tone is in general altered." (2.) "The phase-displacement that occurs during the telphonic transit is not a constant quantity, its amount changes with the constitution of the path of the current, and depends on the number of vibrations." (3.) "In certain cases, however, the amplitude of the induced current becomes independent of the vibration number n and thus the tone of the exciting sound is unchanged." J. T.

II. Geology and Natural History.

1. *Reports upon the Specimens obtained from borings made in* 1874, *between the Mississippi River and Lake Borgne, at the site proposed for an outlet for Flood Waters;* by Prof. Eugene W. Hilgard and Dr. F. V. Hopkins. With a letter of transmittal from Bvt. Maj. General G. K. Warren, Major of Engineers, President of the Commission. Washington, 1878.—This memoir, of great interest geologically, is from the Supplement to the Report of the Commissioner of Engineers of January 16, 1875, on the Reclamation of the Alluvial Basin of the Mississippi.

Prof. Hilgard published, several years since (Rep. Chief of Engineers, U. S. A., 1870), an account of a section 146 feet deep, made in boring a well at New Orleans in 1856. In it, the part below a depth of 41 feet (numbered in the present pamphlet 1 to 4) was found to contain marine shells, and to consist, below the 112 feet level, of a drab or bluish clay (numbered 1), and (Nos. 2 to 4) marine sandy beds, excepting 3 feet of clay (No. 3); then, from 21 to 41 feet in depth, a blue fluvio-marine clay, here numbered 8; and, at top, 21 feet of fresh-water bluish tough clay, here numbered 11.

In the borings of 1874 referred to in the above title, the same beds were met with. No. 1 was reached in four of them at the depth of 91 to 97 feet, and No. 4, below 57 to 72 feet; both were found to contain numerous marine shells, while the thin clay bed No. 3, appeared to be a fluviatile stratum. No. 4 has afforded the shells *Balanus eburneus*, *Nassa acuta*, *Anachis avara*, *Oliva mutica*, *Utriculus* (*Tornatina*) *biplicatus* (*Bullina canaliculata* of Rep. of 1870), *Pholas costata*, *Pandora trilineata*, *Corbula cuneata*, *Mactra lateralis*, *Tellina alternata*, *T. tenera*, *T. tenta*, *Macoma fusca*, *Chione cancellata*, *Ch. cribraria*, *Dosinia discus*, *Lucina multilineata*, *L. costata*, *Arca transversa*, *Pecten dislocatus*, and other species.

No. 8 was met with in four borings near Lake Borgne between the upper levels 25 to 26 feet, and the lower 52 to 56 feet, in

three others, between the levels 22 to 38 and 64 to 73 feet; and varying much in depth in others. This bed is a *blue delta clay*, the deposit of a tide-water marsh, containing no large marine shells, but an intermingling of marine and fresh-water microscopic forms, the latter increasing in number landward. The evidence shows that it was traversed by streams.

Above No. 8, the next marked beds are Nos. 11 and 12, of which No. 11 is a cypress-swamp deposit, and 12, a sea-marsh clay, both still in progress. Dr. Hopkins gives a list of the microscopic organisms of strata Nos. 8 and 12 with notes and figures, and points out the fact that there is a marked difference between them. Among the species in No. 8 there are *Grammostomum Americanum*, *Lenticulum discus*, *Rosalina Beccarii*, *Navicula Gundleri*, *Pinnularia viridis*, *Eunotia gibberula*, *Coscinodiscus radiatus*, *Leptocistinenia Kinahani;* and in No. 4, *Rotalia pachypleura*, *Planulina elegans*, *Navicula fulva*, *N. viridis*, species of *Rhizosolenia*, *Nitzschea*, *Globigerina*, etc. The two have in common *Orbulina universa*, *Melosira distans*, *Cocconema lanceolatum*, *Synedra acuta*. Prof. Hilgard remarks:

"Stratum 8 may be a true delta deposit of the present Mississippi, such as it has been since the re-elevation of the continent which determined the erosion of the present trough of the river. The fact that an apparently fluviatile stratum (No. 3) occurs lower down, may be taken as an indication that both Nos. 2 and 4 fall within the modern period of delta formation. Yet it is not to be supposed that during the period of depression some definite channels, such as that indicated by strata Nos. 2 and 7, did not exist here as well as in the region above, at Port Hudson (Smithsonian Contr. No. 248, p. 5), and elsewhere. But in a 'drowned' delta in course of depression, such channels would, on the whole, be smaller, shallower, and more shifting than during the period of elevation, or the quiescent one that now prevails."

"On the other hand, stratum No. 8 might be regarded as the denuded remnant of a much thicker stratum deposited during the period of depression, and therefore sensibly contemporaneous with the 'Port Hudson clay.' If, as seems probable, it extends to seaward beneath the water of Lake Borgne, it is difficult to conceive it otherwise than as the continuation of the 'blue-clay bottom' of Mississippi Sound, about whose antiquity, and connection with the Port Hudson beds proper, there can scarcely be a question. But until actual examination shall have determined these points, and especially the microscopic similarity of the 'blue-clay bottom' to stratum 8, speculation as to how the latter came to occupy its present position can hardly lead to any useful conclusions."

"It was not, of course, to be expected that borings reaching only to a depth not greater than that sometimes attained by the Mississippi River itself, should throw any direct light on the question of the depth of the delta deposits in the upper delta plain. Yet, in so far as the results of the present investigation

corroborate the steady and rapid increase of the marine character as we descend, as well as an appreciable difference of the fauna from that now ordinarily thrown ashore on the delta beaches, they tend to corroborate also my previous conclusion that the delta deposits proper, at least at and above New Orleans, have a comparatively inconsiderable thickness; and that this anomalous structure of the delta of the great river is in direct causal connection with the equally anomalous phenomenon of the mud-lumps."

The memoir contains, besides a map and sections, three plates of fossils; two by Dr. Hopkins, representing the microscopic organisms obtained from the various beds, the upper as well as the lower, and one of larger fossils by Hilgard. Among the latter Prof. Hilgard names, as probably new species, *Serpula fenestrata*, *Turbonilla undecim-sulcata*, *Dentalium lævе*, *D. sexangulare*, *Cardium æquilaterale*, and *C. inæquilaterale*.

2. *The Question of the Gonidia of Lichens.*—The discussion opened by the well-known Alternative of De Bary (Morph. and Phys. d. Pilze, etc., p. 291), to which Schwendener gave such prominence, and which Bornet has especially illustrated, has been brought at length to what looks like a conclusion by the observations of Dr. Arthur Minks, of Stettin.

The early *dictum* of Fries, that, however related the Lichens may be to the Algæ by their vegetation, they are Fungi as regards their fruit, was brought to mind again, more than fifty years later, by the just cited pregnant remarks of Professor De Bary, on the relation of the Nostochaceæ and Chroococcaceæ to the Lichens, which he winds up with the (freely rendered) following observation. It is scarcely then, he says, to be doubted that a large part of these families of Algæ stand in near genetic relation to the Jelly-lichens, Ephebe, etc. In what relation, it remains to ascertain. Briefly to indicate my opinion, two suppositions appear to present themselves: Either the Lichens we have spoken of are the perfectly developed fructifying states of plants, the imperfectly developed conditions of which ranked heretofore as Nostochaceæ and Chroococcaceæ, among the Algæ—or the groups last named are typical Algæ, which assume the form of Collema, Ephebe, etc., in consequence of being penetrated by certain parasitical Ascomycetes, which spread their mycelial cells through, and thus condition the growing thallus. (De Bary, as above.) Now there appears to be no doubt that these green cells of Lichens (gonidia) are in exactly the indicated relation to the similar cells of the Algæ named, or that the Lichen fruit is equally in accord with certain Fungus fruits—whatever explanation of either fact be attempted. And Professor Schwendener first took up the consideration of the question from this definite ground: If, said he, at the end of the last of his papers on the anatomy of the thallus, in Naegeli's Beiträge, 1868, p. 195, the possibility of such a process as is indicated in the second supposition of De Bary, and in certain cases even the probability of it, can no longer be impugned, the inquiry forces

itself upon us whether it be not possible that all Lichens arise in this way; whether the gonidia be not always to be reckoned typical Algæ, and the colorless thread-cells, in like manner, Fungus-hyphæ. And, after following the consideration of this through several pages he concludes with the remark that, whatever weight be conceded to what he has advanced, it is impossible to deny that the acceptance by way of hypothesis of such parasitic action is authorized by what we know, and that the question deserves, therefore, a thorough investigation. This, Schwendener himself took up the next year (Die Algen-typen der Flechten-Gonidien, 1869), and has also published, besides shorter papers which I am not now able to refer to their places of publication, Erörterungen der Gonidien-frage, 1872, since which he has not returned to the inquiry in print. It has been continued, however, with great interest by others, and if lichenologists have generally looked askance at it, physiologists have done their best, we may say, to show it favor.

Known already among lichenologists by studies of the most sincere and thorough kind—of which I will refer only to his Beiträge z. Kenntniss des Baues u. Lebens der Flechten, pp. 126, and two plates, 1876—Dr. Minks has specially directed his more recent efforts to the resolution of the problem of Schwendener. It had sometimes seemed as if the general, more or less harsh and subjective criticism which makes so large a part of even scientific controversy had been all on one side in this debate, and the pure struggle for objective truth almost altogether on the other; and the interest of unprejudiced observers could hardly fail to attach itself to the last, whether ridiculously aspersed as "notorious" or not. It was yet evident in the *Culturversuchen*, as well of Tulasne as of the later experimenters, especially Reess and Stahl, that elements of uncertainty not conceivably to be eliminated, infected these experiments, and it began to be doubtful whether final results could be looked for by this or, indeed, any other tried way of approach: or, whether the systematic exhibition of Schwendener's hypothesis by Sachs (Lehrbuch, edit. 3, 1873, p. 267) could well expect any satisfactory proof.

Most interesting was it therefore to every student of the Lichens that the keen observer to whom we have referred should buckle to the contest in the most weighty dispute that ever arose in this humble realm of vegetable nature. The long-promised second part of Dr. Minks's Beiträge, with full illustrative plates, has not yet however made its appearance here, and we have only an abstract of the treatise, given by the author in the Regensburg Flora, 1878, to refer to. This is, however, sufficient to indicate the importance of the author's results. It is with the microscopic history of those very minute, often green cells, which have received the name of microgonidia, that this paper is mainly occupied. The cells in question owe their name, as also a recognition of their importance in the question, to Kœrber (Zur Abwehr der Schwendener-Bornetschen Flechtentheorie, 1874), but the whole remarkable exhibition of

their real character and history, to the sufficient microscope, and the patient skill of Dr. Minks; who has thus shown that the lichenologists are quite right, and that the gonidium is plainly a modification of the one (ideal) lichen-cell, in the distinction of which from the fungus-cell it may still be taken for a criterium. The first beginning of the microgonidium, as observed in the hyphæ, is a pale-greenish, broken axial column, passing into irregular strings of rounded masses of protoplasm, which finally acquire the cell-wall and whole structure of gonidia, and escaping from the mother-cell are found free, in every degree of intermediate form between microgonidium and gonidium. No distinction between these two appears, except in size or color; and they characterize in their earliest conditions, we have finally to say, every modification whatever of the lichen-cell, which thus bears witness everywhere (to the sufficiently armed and instructed eye) to its natural autonomy.

Owing in part to the peculiar texture of the lichen he had in hand, Dr. Minks, whose observations were made with a power of about 1250 diameters, laid much stress on the preparation of his material with liquor potassæ and sulphuric acid; but Dr. Müller of Geneva, who has repeated the observations of Minks, and with more powerful objectives (Flora, 1878, n. 31) finds that such chemical preparation is by no means always necessary; to which I can myself testify. Microgonidia were seen by Müller in every part of the lichen structure; namely, in the fibrils of the under side, in the cortical cells, in the medullary cells, in the paraphyses, the young thekes, the spores, the basidia, and the spermatia.

After many unsatisfactory attempts, with dry objectives, and inferior powers, but with some attention to chemical preparation of the material, I have had at last the pleasure, with an immersion $\frac{1}{8}$ of Tolles, to clearly discern the pale-greenish, broken column, passing into rounded, microgonidium-like masses, contained in, and seen at length to escape from, the medullary hyphæ of the Parmelia of Wright Lich. Cub. n. 74 (there called by me *P. tiliacea*, v. *flavicans*, and supposed the same with the *P. relicina*, at least of Montagne) reaching this result with a power of only some six hundred diameters, and without other preparation than a thorough maceration of the tissue in water. With a $\frac{1}{16}$ of Tolles, a 1-inch eye-piece, and power of about 1,000, the whole structure and especially the color was better exhibited; as it was, I need not say, best of all in Tolles's admirable $\frac{1}{16}$ and $\frac{1}{25}$. These observations have all been repeated by my friend Mr. Stodder, with similar results, and I owe entirely to him the manipulation of the two objectives of highest power.

I have only then most heartily to commend to botanists interested, the forthcoming treatise of Dr. Minks, which may soon be expected to appear. E. TUCKERMAN.

3. *Etudes Phycologiques*, by M. GUSTAVE THURET and Dr. EDOUARD BORNET. Fol. Paris, 1878.—This magnificent work surpasses anything which has ever been published relating to

algæ. It comprises fifty-one folio engravings by Picart from drawings of Bornet and Riocreux. Most of the plates were prepared under Thuret's direction between the years 1846 and 1856, and several appeared in a reduced form in the Annales des Sciences of 1851 as illustrations of his article "Recherches sur les Zoöspores des Algues." It was Thuret's intention to publish an atlas of fifty plates, but, at the time of his premature death, ten of the plates had not been engraved. These were finished under the direction of his friend and co-worker, Dr. Bornet. Never before have the algæ been so exquisitely delineated, whether microscopically or in gross. The life-size figure of *Fucus platycarpus* is perfection itself. The text is principally by Dr. Bornet, who has inserted when possible the notes and descriptions of Thuret himself. No apology, however, was necessary on the part of the former; for not only was he the constant companion of Thuret, but his style of writing very closely resembles that of his lamented associate. The text modestly purports to be simply a description of the plates. It is, however, much more: it is a very elaborate exposition of the structure and reproduction of the different groups of algæ. The principal part of the observations on the *Fucaceæ* have already appeared in the Annales. The part relating to the *Phæosporeæ* is very clearly presented, and is the most complete account of the order yet published. The fertilization of *Polyides rotundus* resembles that of *Dudresnaya* in the growth of a number of filaments from the base of the trichogyne. The account of the reproduction in the *Corallineæ* throws a new light on the structure of that order; and for the first time a detailed account is given of the antheridia and cystocorpic spores. W. G. F.

4. *Note on the extension of the coiled arms in Rhynchonella;* by EDWARD S. MORSE.—Years ago VonBuch recorded that Otto Frederic Müller had observed *Rhynchonella psittacea* protrude its arms beyond the anterior borders of the shell. This single observation was not widely accepted, and many doubted the possibility of the arms being exserted in this manner. In the year 1872, while studying living Rhynchonella in the St. Lawrence, I observed a specimen protrude its arms to a distance of four centimeters beyond the anterior borders of the shell, a distance nearly equaling twice the length of the shell. This year I again had an opportunity of studying Rhynchonella in Hakodate, Yesso, and again observed the same features. Specimens lying on the bottom of a glass dish protruded their arms a short distance and remained in this position for hours. A figure is here given representing the appearance of one of them. The movements of the arms were very sluggish though the cirri were constantly in motion. Sometimes the shells closed upon the arms before they were retracted. Lingula has the power of partially protruding its arms, as I have repeatedly observed in North Carolina and Japanese species. Terebratulina can also partially protrude the cirri.

Tokio, Japan, November 26, 1878.

5. *Fauna Littoralis Norvegiæ;* edited by J. KOREN and Dr. D. C. DANIELSSEN. Part III, with 16 plates. Bergen, 1877.—This volume, which, as stated by the editors, has been delayed several years, contains much that is of importance in connection with our own marine fauna, for many of the species, very fully described and figured in it, inhabit, also, the coasts of New England, although in many instances their identity has not yet been pointed out. The text is printed in both Danish and English, in parallel columns. The subjects treated are as follows: New and little known Cœlenterates, by M. Sars; New Echinoderms, by M. Sars; Descriptions of some new Norwegian Cœlenterates, by Koren and Danielssen; Contributions to the natural history of the Pennatulidæ living on the Norwegian Coast; Descriptions of new Bryozoa; Contributions to the natural history of the Norwegian Gephyreæ; a New species of the genus *Pennella* (*P. balænopteræ*). The last four articles are by Koren and Danielssen. Among the Cœlenterata described, are species of *Corymorpha*, *Myriothela*, *Physophora*, *Phellia*, *Zoanthus* and *Alcyonium*. A list of all the Norwegian Pennatulidæ is given, with descriptions of three new genera and six new species of that group. Of the Norwegian Pennatulidæ at least three species inhabit our coast, viz: *Ptilella grandis*=*Pennatula borealis* Sars; *Pennatula aculeata*; *Pavonaria* (=*Balticina*) *Finmarchica*. The *Alcyonium fruticosum* Sars appears to be identical with our common *A. carneum* Agassiz, of earlier date. The *Corymorpha glacialis* Sars is apparently the same as *C. pendula* Agassiz, of later date. Two very different forms, figured as *Myriothela phrygia*, are supposed to represent different stages of growth. The form with gonophores borne on simple elongated, lateral blastostyles is allied to that recently described by me, in this Journal, as *Blastothela rosea*, and may prove to be generically identical with it. The other form has gonophores borne in groups directly upon the hydroid-body. I have dredged a similar form in Eastport harbor. Whether either of these forms, is the original *Lucernaria phrygia* of Fabricius,* is perhaps doubtful. The *Zoanthus Norvegicus* D. and Kor. is probably the same as my *Epizoanthus Americanus*

* We have, on our coast, another form, which agrees better than either of those here referred to, with the Fabrician description. A specimen dredged off Halifax, N. S., in 52 fathoms, 1877, is about three inches (75^{mm}) high, as preserved in alcohol. The tentaculiferous portion is long and slender, densely covered with slender, rather elongated, capitate tentacles. The gonophores are globular, large, (when mature 2^{mm} in diameter) and are borne in clusters of three to ten, on the sides of lateral blastostyles, of different lengths, each of which bears, at its tapering end, a small group of capitate tentacles, unequal in size. These blastostyles, with their clusters of gonophores, cover less than the lower third of the body. The gonophores near the bases of the blastostyles are much smaller, there being usually only two or three large ones. The mature gonophores contain embryos, covered with tentacles, like those described by Sars. (The embryos of *Blastothela rosea* V. are of the same kind, but smaller). The base gives rise to a numerous cluster of short, slender processes, enlarged at the ends, with adhesive disks for attachment. I believe that this is the genuine *phrygia* of Fabricius, and that the forms described by Sars (originally as *M. arctica*) will prove to be distinct, if the figures be correct.

(1864). Of the Echinoderms, *Oligotrochus vitreus* Sars has been recorded by me, in this Journal, as from deep water, off our coast, and I have also dredged it off Nova Scotia (1877).

The genus *Kinetoskias* is established for two very remarkable forms of Polyzoa, both of which have been dredged by us, off the New England coast. One of these (*K. Smittii*) is identical with *Bugula flexilis*, described and figured by me in this Journal, (vol. ix, p. 415, pl. vii, f. 1, 2, 1875) and probably, also, with the *Naresia cyathus*, figured and partially described by Thompson in the Voyage of the Challenger (vol. i, p. 142). The specimens hitherto noticed have all been attached to the summit of a slender transparent stem, which is regarded both by Thompson and by Koren and Danielssen, as a part of the Polyzoan itself. The numerous specimens dredged by me are, also, for the most part, attached to a stem of the same sort, but varying much in size and condition. They occur chiefly on muddy bottoms, in 50 to 430 fathoms, in many localities, *associated with Corymorpha pendula*, and their "stems" appear to be identical with the dead stems of *Corymorpha*, and like the latter, often have many anchoring rootlets arising from the swollen base. In one instance a small *Sertularia* had attached itself to the same sort of a stem, and the Polyzoan had afterwards attached itself to, and invested, both the *Sertularia* and the supposed *Corymorpha*-stem! Therefore I am led to conclude that the "stem" does not form an integral part of the Polyzoan. Nevertheless its structure is, in other respects, so peculiar as to justify its separation from *Bugula*, as a distinct genus. The second species (*K. arborescens* = *Bugula umbella* Smitt) was dredged by us in 1877, off Halifax, N. S., in 110 fathoms, sandy mud. The article on Gephyreæ is a useful monograph of the Norwegian species, several of which are also found on the New England coast. A. E. VERRILL.

III. Astronomy.

1. *Observatory on Mt. Etna.* Letter to the Editors from Professor S. P. Langley, dated Casa del Bosco, Mt. Etna, January 14, 1879.—You may be interested to learn that the proposed Etnean observatory will probably be commenced in the present year on the site of the "Casa Inglese," a hut used by summer visitors to the volcano, and standing at the foot of the cone, at an elevation of over 9,600 feet. The walls and piers will be constructed chiefly of lava and the building is intended to include the "Casa Inglese," and in addition three other rooms, one of which, containing the equatorial, is to have a conical rotary roof of iron; the others are to be used as a kitchen and bedroom. The equatorial is to be of thirty-five centimeters aperture. It will be seen that the new physical observatory is to be well provided instrumentally. It will be under the eminently competent charge of Professor Tacchini of Palermo; and it will have a situation unequalled by any site at present so occupied in the world.

Science is indebted for this prospective benefit, to Professor Tacchini himself, who has long urged upon his Government the paramount importance of an elevated station for the study of solar physics, and has, it may be observed, cited the brilliant results obtained by our own Professor Young at Sherman as his most convincing proof of the advantage of mountain stations. I write in the hope that the example thus set by Italy may find imitators with us. I have been now for some time at a less high but still an elevated station here (Casa del Bosco is a hut ordinarily unoccupied, about 4,500 feet above the sea), engaged in observations, which it may be hoped will be of some use in determining what may be expected in similar sites in our own territory, their aim being to substitute some sort of quantitative data, for our present conjectural knowledge, as to the degree in which the conditions of vision are improved at higher stations, and to form with something of definiteness a standard of comparison. The results (which will probably appear in a report presented to the U. S. Coast Survey) are not as yet complete; but I may say, in general terms, that while as regards observations of precision (perhaps even as regards work on double stars, and like measures), the gain is less than might have been expected, too much can hardly be said of the immense advantage of an elevated station for almost every kind of research connected with solar physics. This is specially the case as regards the chromosphere; while, as to the corona, concededly, our only hope (with our present means) of materially extending our knowledge of it, lies in the prospect that we may yet be able to see it without an eclipse, if the observer be in an exceptionally transparent atmosphere. I will add that, after a recent expedition to Colorado, and with the conditions of observation there and here freshly in mind, I have no hesitation in saying that our own country has sites at the least equal to the proposed Etnean station in every astronomical requisite, and far easier of access. It is most earnestly to be hoped that something will be done with us in this direction *soon*, even if on a very moderate scale. If we wait for such a distant event as the completion of the Lick Observatory, we shall find the laurels gathered by European observers before we are upon the field.

IV. Miscellaneous Scientific Intelligence.

1. *Earthquake of November* 18, 1878.—In regard to this earthquake (already noticed, vol. xvii, p. 158, Feb., 1879), a special Bulletin from Prof. Nipher, Director of the Missouri Weather Service, dated January 1, 1879, gives the following particulars:

"The shock was felt over an area of fully 150,000 square miles. The region disturbed forms an ellipse, the major axis of which extends from Leavenworth, Ks., to Tuscaloosa, Ala., a distance of over 600 miles. The minor axis extends from near Clarksville, Ark., to a point midway between Cairo, Ill., and St. Louis, a distance of 300 miles. The southern boundary of this region has not been determined with as great accuracy as is desired. The region

of greatest disturbance was along the Mississippi, from Cairo to Memphis. Here the shocks were universally felt. The walls of buildings could be seen to move, and strong frame buildings creaked, as when every joint is strained by a heavy wind. At Ironton, the shock was also so severe as to alarm some who lived in brick houses. Along the Missouri, from Glasgow to Lexington, the shock was also severe, awakening many families who thought a wind storm was in progress. It appears that the shock was first felt at Glasgow $11^h\ 23^m$ P. M. (St. Louis time). The shock traveled rapidly down the axis of the ellipse, reaching Cairo at $11^h\ 48^m$ and Memphis at $11^h\ 50^m$. The velocity of transmission is a matter yet under consideration, and will receive attention in a future bulletin. At Little Rock, Ark., the shock was also felt, although not observed at Clarksville, 35 miles farther up the river."

The bulletin is accompanied by a map of the district, on which and within the ellipse referred to above, there are marked twenty-three stations where the shock was felt and eleven Missouri stations where it was not felt. From this map the direction appears to have been N.W. to S.E. instead of N. to S. as stated in our previous notice. C. G. R.

A second Bulletin in regard to this earthquake has been issued by Prof. Nipher. In it he says:—

"According to the few determinations of time made, there were two distinct centers of disturbance, the shock beginning at the one, near Glasgow, Missouri, at $11^h\ 23^m$ P. M.; at the other near Paducah, Kentucky, at $11^h\ 34^m$ P. M. (St. Louis time).

The following times are deemed reliable:

Glasgow Region.		Intermediate.		Paducah Region.	
Glasgow	11^h23^m	St. Louis	11^h57^m	Paducah, Ky.	11^h34^m
Leavenworth	11–34	Ironton	11–45 to 11–50	Charleston, Mo.	11–45
Lexington	11–38	Lebanon	12–19	Cairo, Ill.	11–48
Macon	11–38	Little Rock	12–13	Memphis, Tenn.	11–49

With so few data, it is only possible to give approximate determinations of velocity, as the wave fronts can not be determined with precision. The average velocity was probably less than 200 miles per hour. In some regions the velocity was as low as 160 miles per hour.

Direction of vibrations: Paducah, N.W.–S.E. (Chandelier), Cairo, W.N.W.–E.S.E., Charleston, N.–S., Little Rock, E.–W., Glasgow, N.–S., by some, N.W.–S.E. by others. At Ironton the rumbling sound accompanying the shock, seemed to go *from S. to N.* A similar sound was heard at Gayoso, Missouri, and Memphis, Tennessee. The shock seems to have been more violent in the New Madrid region as far south as Memphis, than in the Glasgow region."

The Bulletin is accompanied by a map, showing the regions affected as above, and also the neighboring field of the earthquake of Nov. 15, 1877. C. G. R.

2. *Forschungen auf dem Gebiete der Agrikulturphysik.* Herausgegeben von Dr. E. WOLLNY, Professor der Landwirthschaft in München. Band i, 482 pp., and Band ii, 1 and 2 Hefte. Heidelberg: C. Winter.—This excellent journal of agricultural physics, which has entered on its second year, fills a very necessary place in scientific literature. Some of the more important questions in agriculture are largely or purely physical, and demand special study as such. Their investigation will be greatly stimulated and promoted by the existence of an organ in which the hitherto scattered results of research are brought together. The seven numbers of this journal before us contain not only valuable original papers on various subjects belonging to the physics of the soil, the plant and the atmosphere, but in the careful discussions of older investigations, give instructive reviews of nearly all that has been done in several branches of these subjects. The contributions of the editor, Dr. Wollny, on the influence of the color, exposure and texture of the soil, on its temperature, are models of experimental investigation. The original articles are supplemented by abstracts of papers elsewhere printed, and by lists of publications. S. W. J.

3. *The American Journal of Otology:* a quarterly Journal of Physiological Acoustics and Aural Surgery; edited by Clarence J. Blake, M.D., in conjunction with Prof. A. M. Mayer, Dr. Albert H. Buck, Dr. Samuel Sexton, Dr. C. H. Burnett, Dr. J. Orne Green and Dr. H. N. Spencer. Vol. i, No. 1, January, 1879. New York (William Wood & Co.).—This new journal proposes to fill a new and highly important place among American scientific periodicals. The first number contains among others an article on the graphic and photographic illustration of Sound-waves, by Dr. C. J. Blake.

The following books have been received but cannot be noticed in this number:

Wanderings in South America, the North-west of the United States and the Antilles in the years 1812, 1816, 1820 and 1824, by Charles Waterton; new edition, edited by the Rev. J. G. Wood. 520 pp. 8vo. London, 1879 (Macmillan & Co.)

Journal of a Tour in Morocco and the Great Atlas, by Joseph Dalton Hooker and John Ball; with an Appendix by George Maw. 499 pp. 8vo. London, 1878 (Macmillan & Co.)

The Study of Rocks: an elementary Text-book of Petrology, by Frank Rutley, F.G.S. 319 pp. 12mo. London, 1879 (Longmans, Green & Co.)

Ordnance Notes, No. xc. Measurements of Powder Pressures in Cannon by means of the registered compression of oil: experiments by Dr. W. E. Woodbridge, at Washington Arsenal in 1854–55. Washington, Nov. 20, 1878.

Revue de Géologie pour les Années 1876 et 1877; par M. Delesse et M. de Lapperent. Vol. xv, 229 pp. 8vo. Paris, 1879.

Geological Survey of Pennsylvania: Report of Progress in the Juniata District on the Fossil Iron Ore beds of Middle Pennsylvania, by John H. Dewees; with a Report of the Aughwick Valley and East Broad Top District, by C. A. Ashburner. 305 pp. 8vo. Harrisburg, 1878.

Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass. Vol. v, Nos. 8, 9 and 10. Reports on the Dredging Operations of the U. S. Coast Survey steamer "Blake." Description of Sounding-machine, water-bottle and detacher, by Lieut.-Commander C. D. Sigsbee, U. S. N.; pp. 169–179. Echini, by A. Agassiz. Corals and Crinoids, by L. F. de Pourtalès. Ophiurans, by T. Lyman; pp. 181–238. Hydroida, by S. F. Clark; pp. 239–252.